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Science and Technology

(From the Month's News Releases)

Germ-killing Paper

Paper can now be successfully impregnated with a lasting germ-killing or repelling process and product. Not only does the process and product kill or inhibit a wide range of disease-producing bacteria, but it is effective against certain molds and fungi. Application to paper can be made to several of the wet stages of paper production, and also has been imparted to various types of pulp and to some kinds of finished paper without any apparent change to the physical characteristics of either. The use of this development in paper products such as toilet and facial tissues, paper napkins, paper diapers, drinking cups, food containers, soda straws, surgical masks, wrapping paper, and many other paper objects offers promising possibilities.

Tester

In a new cloud and pour point tester, as many as four oil samples may be tested simultaneously, at temperatures down to $-90°$ F. Designed to meet the specifications for ASTM test D97, the apparatus consists of a sheet-metal cylinder with a heavy stoneware liner, insulated on sides and bottom with rock wool. This container is the bath in which appropriate cooling mixtures are held. A heavy hardwood cover has an enlarged central opening through which shaved ice or pellets of solid CO_2 may be dropped to replenish the cooling mixture.

Paper Cutter

A self-sharpening rotating wheel blade, enclosed in a protective cast aluminum carriage, travels on a solid I-beam track and trims paper, film, clippings, drawings, and photographs. The board may be left completely unprotected, as there is no knife to cut or pinch fingers. Available in sizes from $12'' \times 12''$ to $8'' \times 42''$, with other sizes on special order.

Wax and Kill

A wax that makes floors shine and at the same time kills roaches, ants, water bugs, silverfish, and other household insects is self-polishing and water-resistant. It may be used on linoleum, asphalt and rubber tile, and cement, as well as on varnished and painted wood floors. It is also effective in areas where insects run, such as cracks, crevices, baseboards, pantry shelves, around sinks, pipe openings, and floor and window sills. One application lasts for about six weeks.

Insect-killing Light Bulb

An insect-killing light bulb has recessed pockets in which pellets of an insecticide are vaporized. Designed for home, commercial, or camp use, the light bulb works in all positions and should burn more than 8000 hours.

Fire Blanket

A new type of vinyl-coated glass cloth fire blanket is approximately 30 per cent less expensive than a conventional wool fire blanket. The protective blanket can be thrown over a person or machine enveloped in flames, or can be used in smothering fires at vents, manholes, barrels, buckets, and other containers. The vinyl plastic coating provides resistance to acids, alkalies, most solvents and other chemicals, stays flexible to below zero Fahrenheit temperatures, and will not support combustion.

Toxicity Determination

A simple method for determining quickly the toxicity of various beryllium oxide dusts that have caused acute pneumonitis when inhaled was demonstrated to industrial hygienists and physicians attending the 1951 Health Conference in Los Angeles. A "dispersion staining" technique is used, employing polarizing and dark-field microscopes. The most dangerous beryllium oxides appear as dark particles with the polarizing microscope, and as a mixture of light blue and white particles in a dark field. The less toxic manufactured grades appear as bright particles with the polarizing microscope, and bluish red with the dark field. Beryllium oxide, like silicon dioxide, the causative agent of the lung disease silicosis, can apparently exist in several stable forms which vary in their ability to cause acute pneumonitis.

Portable TV Microwave Equipment

Two new developments in television equipment have just been released. One is the world's most portable TV broadcaster, operating on a frequency of 7000 megacycles. The other is the most powerful TV microwave relay equipment, operating on a 2000 megacycle frequency. The latter is designed to pick up picture and sound simultaneously in remote places and shoot them back to the studio for telecasting, all without the use of wires. The first device is the nearest thing to a vest-pocket television broadcasting station, for it is light enough for one man to carry. A preliminary model of the second device was used to transmit the first recording of an atom bomb explosion in Arizona. It is possible, across favorable terrain, to transmit over distances greater than 100 miles in a single hop. These devices are the first to handle pictures and sound simultaneously on the same waveband. The picture and the sound, arriving as a single signal, are "unscrambled" or separated electronically by the receiving end of the equipment, and are then sent out on the proper wave lengths for reception by home TV receivers.

Address a post card to Science and Technology, 1515 Massachusetts Ave., N.W., Washington 5, D. C., for further information about any item on page iv.

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Public Education in the Philippines—Footnote to the Future*

J. CAYCE MORRISON

In 1949, at the request of the government of the Philippines, Unesco sent a Consultative Educational Mission to the Philippines to make a survey of the nation's public schools. Following publication of the mission's report, in 1950, the government of the Philippines asked Unesco to send a technical adviser to assist the government in implementing Unesco's recommendations on educational administration and finance. Dr. Morrison was given this assignment. His proposals for implementing Unesco's recommendations of 1949 are embodied in a bill entitled "A Foundation Program for Financing Public Schools in the Philippines." The proposals have the endorsement of the Department of Education and the Philippine Association of School Superintendents. The proposed bill is before the appropriate committees of the Congress of the Philippines for consideration during the 1953 session.

On August 23, 1901, the first contingent of American teachers landed in Manila. On July 4, 1946, the Republic of the Philippines took its place among the nations. Between those two dates American teachers had helped a willing people to develop the qualities of responsible citizenship. In less than half a century a people inhabiting a thousand isles, divided by nearly eighty languages, the victims of three centuries of exploitation, had found unity in an alien tongue and accepted responsibility for governing themselves. Nothing quite like this had ever happened before in the history of peoples. Probably in no other country is there such an excellent opportunity to observe the basic contribution of free public education to developing capacity for citizenship and self-government.

To understand what is happening in the Philippines, it is important to understand the longing of the Filipinos for education. This was vividly expressed by José Rizal,[1] more than ten years before the Americans arrived:

> The school is the basis of society. The school is the book in which is written the future of nations. Show me the schools of a people, and I will show you what that people is.
>
> I desire the country's welfare; therefore, I would build schools.

Almost without exception the Filipino leaders

* Based on the address of the retiring vice president, Section Q, at the Annual Meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, St. Louis, Missouri, December 30, 1952. The paper is also appearing in the *Philippine Educator*.

sought to keep their schools alive, to free them from clerical control, to make education compulsory, and to establish public schools secular and free. These ideals found expression in the four great constitutional documents of the revolutionary period.[2]

Before the Americans arrived in the Philippines, under the Spanish Education Act of 1863, approximately 1000 primary schools had been established, with an estimated enrollment of about 200,000. It was on this foundation that the Americans started to build.

Legal Basis of Public Schools

The legal basis of the present public school system in the Philippines was established by Public Act No. 74 of the American Commission in January 1901. Except for Commonwealth Acts of 1938 and 1940, the structural changes, to date, have been minor.

In 1938, the Congress of the Philippines enacted a law known as Commonwealth Act 381. This reaffirmed the responsibility of the national government for financing primary education (Grades 1–4), but shifted responsibility for intermediate education (Grades 5–7) to the chartered cities and municipalities. In order that they might carry this responsibility, the Congress required each local authority to levy a tax of 2/8 of 1 per cent on the assessed value of real estate, and made available to the local governments certain additional tax resources for the support of schools.

Finding that many of the municipalities could not maintain their intermediate schools under the provisions of C. A. 381, the Congress, in 1940, repealed the act and substituted for it C. A. 586. The latter carried over the provisions of C. A. 381 as they applied to chartered cities, but transferred to the national government responsibility for financing intermediate schools in the municipalities. As an economy measure the Congress, by C. A. 586, abolished the seventh grade, limiting the intermediate school to Grades 5–6. The debates on this measure resulted also in pressure on the Bureau of Education to experiment with the one-session, or half-day, program in the elementary school.

The War Years

The school year 1940–41 opened with about 2,000,000 children enrolled in the public schools. English was the chief language of instruction in all schools. In general, public secondary schools had been established only in the provincial capitals and a few other populous centers. They were supported in considerable measure from provincial funds and in some cases were tuition-free. The census of 1939 had reported approximately 50 per cent of the population above ten years of age as literate.

The Japanese entered Manila on January 2, 1942, and by the following June were in control of most of the provincial centers in the Philippines. Wherever they took control, the schools were made an instrument of propaganda. Children left school either from the necessity of escape with their families or as a patriotic duty. In many centers the Japanese established their headquarters in the public school buildings, which then became objects of attack by guerrillas and later by the Americans in attempting to dislodge the enemy. When the liberation from the Japanese came, nearly half the public school buildings had been destroyed, books were gone, teaching staffs were scattered, the new government was yet to be formed, and the country was desperately impoverished. Between January 1942 and April 1945, enrollment in the public elementary schools dropped from about 2,000,000 to about 200,000.

Wherever the American Army moved in to give protection, however, the first act of the local government was to open the schools. There was no money, no material for permanent buildings; there were few books and few qualified teachers available; and for three or more years children had been subjected to all the demoralizing influences of war. The national government agreed to provide a teacher wherever the people would provide a classroom. For the most part, these temporary classrooms were erected on school sites; they had woven bamboo walls, nipa shingle or thatched roofs, dirt floors, and open spaces for windows with wooden shutters. Teachers and pupils found books, paper, and pencils where and as they could. By July 1946, most of the children who had left school wanted to return. Such briefly, was the situation in public education when the new government took over on July 4, 1946.

First Six Years Under the Republic

As of July 4, 1952, public schools in the Philippines had completed six years under the Republic. No significant change had been made in their legal structure since the Commonwealth days. But significant gains had been made, and even more promising developments were in prospect.

The total annual enrollment of public elementary schools, Grades 1–6, had reached 4,000,000, with an average daily attendance of over 3,600,000. It was estimated that 80 per cent of all seven-year-old children were entering the public primary

schools and that 50 per cent of those who entered the first grade would, under present conditions, stay in school to the sixth grade. The War Damage Commission had restored a large percentage of the school buildings destroyed in the war, and local governments with the help of parents and other citizens were continuing to build temporary classrooms where needed. In 1948, teachers' salaries had been increased. Most teachers had satisfied the requirements of certification, and few new teachers were being employed who had less education than graduation from a two-year normal college course.

A number of current developments in the work of the public schools give us confidence in the future of the Philippines. These changes are finding expression as phases of the school community program, which may become the chief contribution of the Philippines to education. The school-community program has the endorsement of the Bureau of Public Schools and of the entire Department of Education. Superintendents are encouraged to promote the program, each in his own way, in terms of the needs and resources of the city or province he serves. This has led to the coordination of major educational objectives into one program, which is displaying new vitality throughout the country. In general, the school-community program is designed to improve the economic status of the people, reduce illiteracy, improve public health, develop civic consciousness, and train for the responsibilities of citizenship in a democracy.[3]

The school-community program is commonly initiated by a survey of the community, conducted by the teachers and children. Each teacher is given an area. She visits the homes, talks with the people, and reports the facts concerning the number of people, the degree of their literacy, the character of the homes, the sanitary conditions observed; the economic features of the home, such as livestock, poultry, and garden; and aesthetic features of the neighborhood that might be improved. Through the children, visits to the homes, and parents' visits to the school, steps are taken to improve conditions where such improvement may be achieved by the school's participation or its dissemination of knowledge.

The school-community program is giving a new impetus to the effort of the schools to increase the subsistence level of the common people. From the beginning, American teachers emphasized the development of school gardens. Under the Republic, the schools are expanding and giving new vitality to school gardening both as a method of education and as a means of raising the economic status of the people. This expansion takes various forms, depending, in part, upon the occupation of the people, the fertility of the soil, the prevailing climate, and the vision of those in charge. In one province, where banana culture had been more or less haphazard, the school children, in one year, set out 300,000 banana trees. In every community visited in that province the school banana plot was the best cultivated banana planting in the area. The intent is that the knowledge gained in school will be carried over into the care of banana plants in home gardens, and ultimately to the improvement of banana culture on a commercial basis. In another area visited, where the climate is specially well adapted to the growing of flowers, the schools have actively promoted their culture. Almost every home, however humble, has its own flower garden, and collectively these home gardens have become part of an industry that supplies Manila, hundreds of miles away, with flowers the year around. The most prevalent extensions of the school garden are piggery and poultry projects. The schools are obtaining from the state agricultural colleges good stock, not only demonstrating what can be accomplished through better breeding and scientific feeding, but also serving to improve the stock in the area served by the school. In mountainous areas the schools are beginning to establish goat farms. Along the coast, the schools are establishing fish ponds and hatcheries. Through showing the people how to do better the things they were already doing, the schools are helping to improve the economic status of the people of the Philippines. To the achievement of this goal, the Mutual Security Agency of the United States is making an important contribution by helping the government of the Philippines to expand and equip the vocational schools at all levels. The influence of this contribution is reflected in the work of the intermediate and primary schools.

In the school-community program, emphasis is also placed on community improvement. As teachers and other adults gain experience in working together, the school becomes a center for adult groups to discuss community problems and to consider ways and means of solving them. Much consideration is given to improving sanitary conditions, both in the homes and the community at large. Reading centers are established, in part as a means of promoting social intercourse. Consideration is given to aesthetic improvements. Improving conditions in the barrio or the municipality is the beginning of practical training in citizenship. The importance of this movement is that pupils and parents, teachers and citizens, are working together and that the school is the center of a movement

that has immense possibilities for raising the status of an entire people.

In some provinces, the school-community groups are organized to study the theory and practice of democracy at the local level. In many places, the provincial governor, the municipal mayors, and councilmen participate actively in the movement. The writer attended a conference of the leaders of school-community groups in one of the larger municipalities, where the chief speakers were the governor of the province, the mayor of the municipality, and the division superintendent of schools. The discussion, carried on in Tagalog, covered local taxation, municipal finance, obtaining civic improvements such as artesian wells, appraisal of the promises and performance of their representative in Congress, and so on. The conference was held in a large central elementary school. Time was scheduled for the delegates to visit elementary classrooms, where each class had brought, or was in process of bringing, to conclusion a term study of some important subject or problem in historical, civic, social, or economic development of the community, province, or nation. Here was a social studies program in the school, intelligently articulated with a community program that enlisted the active participation of adults, that was directed to helping old and young grow in understanding and practice of the way of life set forth in the Constitution and the declared policy of the Republic.

One would be daring indeed to predict the influence of the school-community program on the future development of citizenship in the Republic of the Philippines. There are observable beginnings that promise well. In one province, at least, many of the Huks have established their families in settled communities where their children can and do attend the public school. In a quiet way the schools are moving to win the hearts and minds of the children, and there is hope that they may win the confidence of the fathers as well.

The language problem has been and still is a deterrent to the development of a literate and intelligent citizenship in the Philippines. The 1948 Census reported about 60 per cent of the population above ten years of age as literate. Since 1901, except for the period of the Japanese Occupation, English has been the language of instruction in the schools and of the government. Under the Commonwealth, Tagalog was declared the basis for development of a national language. The practical effect of this legislation thus far has been to make Tagalog a required subject of instruction, one period daily for all pupils in all schools. But since there are about 80 different native languages and dialects in the Philippines, the development of a native language based upon Tagalog is progressing slowly. During the past four or five years an experiment has been conducted in the province of Iloilo, in the use of the native dialect as the language of instruction in Grades 1 and 2, introducing the use of English at the end of Grade 2 or the beginning of Grade 3. Emphasis is placed on drawing the parents into the school, encouraging the children to tell their parents about the things they learn in school, and encouraging parents to learn to read with their children. Experience to date indicates that by the end of the fourth grade, the children can use English quite as well as others of equivalent ability who were under English instruction during the entire four years. In addition, the children have gained some skill in reading the native language, and the school has developed a closer bond of understanding with the parents. The school-community program, thus makes a reform in the teaching of language a means of reducing illiteracy among adults, thereby drawing them into closer support of the schools.

A Proposal for Financing Public Schools

Not the least gain of the post-liberation period has been the achievement of a general agreement as to what are the current educational problems of the Philippines. This agreement has been reached through the work of the Joint Congressional Committee[4] on Education 1948-49, the survey conducted by the Unesco Consultative Educational Mission[5] of 1949, and by the continuing activities of temporary committees on education. The needs and the problems reported by these two major survey groups may be briefly summarized as follows:

a) *All classes in the primary schools should be placed on a two-session, or full-day, program.*

In 1951–52, about 60 per cent of all primary classes were on a one-session, or half-day, program.

b) *Elementary schools should be made free and available to all children, and compulsory education should be enforced to the completion of the intermediate school or till the age of fourteen.*

The present compulsory education law applies only to the primary school, Grades 1–4, and is not strictly enforced. In the sparsely populated areas there are some municipalities and many municipal districts that have no public intermediate school and a few that have no primary school.

c) *The seventh grade, abolished in 1940, should be restored.*

Both the Unesco Consultative Mission and the Joint Congressional Committee recommended that the seventh grade be restored to the intermediate school. However, a bill introduced in the 1952 session of the Congress of the Philippines proposed making the seventh grade the

first year of a five-year high school program. Unesco's technical adviser in 1952 approved the original recommendation for the following reasons: (1) public intermediate schools are much more widely distributed and, therefore, are more accessible than public secondary schools; (2) since secondary schools are currently financed chiefly from tuition fees, the number of sixth-grade graduates obtaining a seventh year of instruction in secondary schools would be much less than in the intermediate school; and (3) the program of the intermediate school is much better adjusted to improving the social-economic life of the people than is the current high school program.

d) *As rapidly as the resources of the country will permit, public secondary education should be made free and available.*

As of 1950–51, the average daily attendance in Philippine secondary schools was about 425,000. Of these, approximately 183,000 were in public and 242,000 in private secondary schools. It was estimated that a little less than 25 per cent of the youth of high school age were in secondary school, public and private. In the public secondary schools about 85 per cent of the cost of current expenses came from matriculation and tuition fees. The national government made some contribution to the support of vocational education.

e) *Reduction of class size.*

As of 1951–52, the maximum class size was fixed officially at 60 pupils. The average class size was 46 in primary grades and 40 in intermediate schools. There was general agreement that the maximum should be reduced to 40 as soon as possible. The Joint Congressional Committee endorsed a proposal for special teachers in the intermediate schools that would reduce the pupil-teacher ratio to 24–27 pupils per teacher.

f) *Levy of a local property tax in partial support of public schools.*

The Unesco Consultative Mission thought that, ultimately, 50 per cent of the cost of public schools might be financed from local taxes. The Joint Congressional Committee adhered more closely to the tax provisions previously noted in Commonwealth Act 381.

To solve the foregoing and other related financial problems the Department of Education, with the assistance of the technical adviser provided by Unesco, has drawn an Act entitled, "A Foundation Program for Financing and Improving Public Schools in the Philippines." The main provisions of the proposed bill are as follows:

1. Each chartered city and municipality would be required to levy a minimum specified tax on real estate, the proceeds from which would be used exclusively for the support of elementary schools. The municipal governments would be given the same additional tax resources for the use of schools that are now available to the chartered cities.

2. The national government would pay the difference between the cost of the foundation program and the income from the required or minimum local real estate tax.

3. The local governments would be permitted to levy additional taxes for the purpose of improving or extending the foundation program.

4. When half or more of the local taxing units had provided an improvement or extension, the national government would add such feature to the "foundation program," thereby making it available to all the children of the country.

5. The provinces and chartered cities would be required to levy a token tax of ⅛ of 1 per cent and would be permitted to levy an additional tax on real estate for the support of secondary schools.

6. The national government would reserve at least half the real estate tax potential available to local governments for the support of public schools.

The "Foundation Program for Financing Public Schools in the Philippines,"[6] if enacted into law, would be a first step in developing a permanent partnership of the national and local governments in the support of public schools. It would create a more effective working relationship between the schools and the provincial, city, and municipal governments. It would enable the government of any local unit to provide a better program than that mandated for all the schools. Over a five-year period, it would greatly strengthen the elementary school program and establish the concept that ultimately secondary schools should be free and available to all youth. It would provide an incentive to parents and others, through channels now available, to participate in the improvement of their schools and ultimately to achieve more effective participation in the planning and management of schools at the local level.

Back of all proposals for increasing the cost of public schools in the Philippines is the question of economic resources. Currently, the situation is encouraging.

Since 1946 there has been a steady growth in the national income of the Philippines. In the six-year period 1946–51 the gross national income nearly doubled. The gain for 1951 over the preceding year was about 20 per cent. In 1951 there was no appreciable increase over 1950 in the cost of living. There was substantial increase in national income from trade, manufacturing, and other sources, as well as from agriculture. Many factors may alter the trend in national income, but there are many signs that the upward trend will continue.[7]

There is deep loyalty throughout the Philippines to the purposes and methods of the American public school. Even so, there is growing realization that much of the content of courses of study and textbooks must be developed out of Philippine experience and be oriented toward solving Philippine problems. This realization of need is finding expression in the leadership of superintendents in their respective cities and provinces and in collective efforts expressed through their national association. The spirit of this leadership was well expressed by J. C. Laya, in the preface of his book *Little Democracies*.[8]

This is a book on education . . . , but it is also the diary of a creative spirit who, being inexperienced and unbroken, took the tolerance of his Director for encouragement and went out on a daring and somewhat strange search that took him through the minds and hearts and purposes of the people of an entire province. This is the story of the wonderful things he found out at the first leg of his journey. The adventure has just begun. It is an unfinished story.

One thing is sure: It is a tentative record of a fast-changing scene, a freezing of a moment that as we look on is fading into other patterns and other shapes. But even if it is evanescent it has the permanence of the recorded past and will be useful as a point of reference when we plan for the future.

For J. C. Laya, the unfinished story was ended by his untimely death on August 3, 1952; but the story he recorded will be a point of reference for hundreds of men and women in the public schools of the Philippines who will continue to write in their daily work the story of how public schools helped to create a republic and shape its service to its own people and to the world at large.

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ASTRONOMERS

Copernicus and Ptolemy
Had much thought for infinity.
They had a universe to chart,
A room where man might play a part.
The double suns and clusters, spheres,
Became a part of their swift years.
Their world was smaller and had bounds:
In brief, they stood on firmer grounds.
For they knew not as much as us—
Old Ptolemy, Copernicus.

Daniel Smythe

Delanson, New York

Creative Technology*

EARL P. STEVENSON

Earl P. Stevenson, president of Arthur D. Little, Inc., a consulting research and engineering organization, entered its ranks in 1919 as director of research. He became vice president in 1935, and since that date has held his present position. During the war years Dr. Stevenson was Chief, Chemical Engineering Division, National Defense Research Committee, and is now a consultant to the Department of Defense and the Chemical Corps. He is president of the Board of Trustees, Wesleyan University, and a member of the board of the National Science Foundation.

WITHIN my lifetime the greatest revolution in the long history of mankind has occurred. Had the prophet Habakkuk been our reincarnated contemporary, he might have been inspired to exclaim as of yore, "Behold ye among the heathen, and regard, and wonder marvelously; for I will work a work in your days, which ye will not believe, though it be told you." This might well be my theme, for it summarizes the achievement of creative technology, which is so largely responsible for the position of the United States today in world affairs. The quickening translation of new scientific knowledge into terms of useful devices is largely due to the development of a system for utilizing the results of science—a system of teamwork among scientists, engineers, and manufacturers.

This new methodology has come into being in the past twenty-five years. It had its beginning after World War I and began to take root as an idea in the twenties, but its growth was greatly retarded by the depression. The unprecedented technological demands of World War II provided the impetus, and the accumulated basic scientific knowledge, the nourishment, for the postwar growth of the process which integrates science and engineering.

The electrical and process industries are probably the major beneficiaries. Within my working years, the modern catalytic cracking plant has evolved out of the Burton invention. The development of 100-octane aviation gasoline was not the accomplishment of one man; it depended upon an application of fundamental knowledge in the field of organic chemistry. This gasoline is a synthetic product and not simply the result of breaking down large molecules to form smaller ones under thermal shocks. The gasoline we use in our cars today is composed of 90 per cent "new" molecules.

The chemical industry owes much of its present glamour, and certainly most of its achievements, to research and engineering. Within the past fifteen years the pharmaceutical industry has also become the beneficiary of these same developments. With the advent of the sulfa drugs, chemical therapy took a new lease on life in the promise of new specifics. The antibiotics brought about another revolution in this age-old industry. Industrial research in its new and broader conception has played a vital role in translating the laboratory disclosures of Fleming and Florey into practice.

The story of the magnetron—the tube that is the heart of radar—offers another example. As far back as the 1920s, two American scientists, working on the basis of Hertz' discoveries, found that distance to an object (in their case, the height of a specific atmospheric layer) could be measured by bouncing radio waves against it. In the 1930s, Sir Robert Watson-Watt, in charge of a small research group carrying out further measurement studies, suddenly noted that aircraft overhead also gave reflected signals. Quickly huddled under a British Air Ministry security blanket, a special research team developed practical installations: the British chain of early warning stations of 1939—which swayed the Battle of Britain in England's favor. As one Englishman has put it, "Britain had fewer fighters than the enemy—but knew where to put them."

The great advance in radar began, however, with the cavity magnetron of Randall and Booth, the prototype of which they put together with "strings and sealing wax" in a university laboratory. Sir Henry Tizard brought a prototype to America in September 1940, and within six months magnetrons were in full-scale production here. This is not merely a story of mass production, but an example of enthusiastic creative teamwork.

* Based on an address presented in the Symposium on Methodology in Engineering Research at the Annual Meeting of the AAAS held in St. Louis, December 26–31, 1952. The paper will appear simultaneously in *Mechanical Engineering*, journal of the American Society of Mechanical Engineers.

The development of the atomic bomb is probably the outstanding example of what can be done when creative effort is organized and focused upon a single objective—when engineers and scientists work closely together as a team to bridge the gap between scientific knowledge and engineering requirements.

Organized creative technology falls easily within the framework of our American political and social concepts and reflects our way of life in giving full play to the genius of the individual, in giving him freedom to exchange ideas with his fellows, and opportunity without artificial restraints for joining with others in creative work. European observers today note what that astute traveler Alexis de Tocqueville remarked about American democracy in the mid-nineteenth century: that the home training of the American child, and his subsequent schooling, engender in him an attitude toward community action that knows no counterpart elsewhere in the world. The American has a strong sense of individuality, but at the same time an urge to work with others in the solution of a common problem. The concept of civic responsibility has nurtured in the typical American an ability to act responsibly and creatively in a group—and without government decree as the motivating force.

Against the background of these introductory remarks, I should now like to develop my theme. At the outset, the methodology of engineering research requires definition. I shall assume for the purposes of my approach and limited traverse that this may be translated, in one-syllable words, into "how to get things done"—meaning to develop new techniques, processes, equipment, or products.

The term "engineering research" requires particular attention. Narrowly construed, it might mean the determination of more exact and comprehensive values and the measurement of important properties of materials in terms of specific uses. Even this restricted definition is evasive and debatable, leading as it does to the conclusion that most physical research is of this kind. It can be pointed out that, since no new laws of thermodynamics, for example, have been discovered since the pioneering work of Helmholtz, Kelvin, and Gibbs, all research in the domain of thermodynamics is, a priori, engineering research. I am sure, however, that this apparently logical conclusion would not be generally accepted.

Definitions can become an ordeal in semantics. Understanding must be reached in practice and not in terms of abstract definitions. As a general proposition, I conceive the area of engineering research to be the gap between the knowledge and understanding supplied by fundamental research and that required by the engineer in undertaking to carry out a definite assignment.

As R. E. Gibson presents the position of the scientist:

> Scientific research in its most elemental form is a very private occupation which eludes all attempts to bring it under control of conventional management. The rule for the organization of pure research is, "Don't try to organize it." It is concerned with *problems.* A research problem grows in the mind of one man when what he knows from experience, intuition or doctrine conflicts with what he observes. To resolve this conflict, he must analyze the experience he has acquired or the doctrine he has inherited, and reassure himself of their consistency and sufficiency; at the same time, he must refine the processes of his observation to insure that he really knows what he is measuring and that he has observed a *reproducible scientific fact.* This in a few words describes theoretical and experimental scientific research. When a new phenomenon is explained in terms of the system of the entities and logic which has grown gradually under repeated analysis and criticism since the time of Bacon and Newton—the system we call for short "scientific knowledge"—we say that we understand it.†

In this spirit the scientist is constantly re-examining old precepts as new problems arise through the study of nature, in the writing of books, in the teaching of students, or in the practice of the useful arts. The theoretical framework of science is seldom radically modified, but rather extended. Einstein's speculations did not invalidate the Newtonian laws of gravitation when these failed to comprehend certain observable phenomena. In this unorganized and highly individualized effort, the great edifice of scientific knowledge has been created.

Where basic scientific research might be said to end, engineering research begins. Increasingly, the engineer and the scientist appear to operate in the same area; their guiding philosophy is quite different, however. The difference in objective is in the "think" and the "thing." An example will serve to clarify this idea.

Take the scientific problem of *ortho* and *para* hydrogen, for example, particularly as it relates to the design of a system for production and storage of liquefied hydrogen. Peculiarly enough, theoretical physicists had predicted on the basis of the quantum theory that two different kinds of hydrogen molecules should exist and should have quite different physical properties—particularly at low temperatures. This peculiarity arises from the requirement that the thermal energy of rotation of molecules is quantized or, in other words, the de-

† Address to Spring Conference, Society for Personnel Administration, May 1951.

gree of excitation can have only certain discrete values—called energy levels. Furthermore, some molecules can have only odd energy levels and the others can have only even ones, depending on whether the two nuclei in the molecule have their spins parallel or antiparallel. This gives rise to two different kinds of hydrogen molecules, those with even energy levels and those with odd, known as ortho and para, respectively. Chemically, the two forms are of course indistinguishable, but physically they are considerably different—different heat capacities, heats of vaporization, enthalpies, and entropies. It might be thought that normal hydrogen at room temperature would contain nearly equal amounts of the ortho and para forms, but this is not the case. At room temperature and higher, normal or "equilibrium" hydrogen contains 75 per cent of the ortho form and 25 per cent of the para. As the temperature is lowered, the equilibrium composition slowly changes and is substantially 100 per cent para at the liquefaction temperature (20° K). If, however, ordinary hydrogen is cooled and liquefied, it retains its room-temperature composition unless a suitable catalyst is present to allow the conversion reaction to take place. In the absence of such a catalyst, normal hydrogen will liquefy with a composition of 75 per cent ortho and 25 per cent para; then, over a period of days, it will slowly convert to the equilibrium state of substantially 100 per cent para, liberating a considerable amount of heat. The heat of conversion is actually greater than the heat of liquefaction, and consequently freshly liquefied, unconverted hydrogen evaporates very quickly, even if it is contained in a perfectly insulated vessel.

I mentioned earlier that theoretical scientists had predicted the existence of ortho and para hydrogen before such a thing was found in nature; indeed this would seem to be one of the most convincing achievements of the quantum theory. It remained for the experimentalists to verify the prediction and to explore the exact consequences of the peculiarity.

But the knowledge thus gained is not sufficient for the engineer who is interested in liquefying and storing hydrogen with minimum power expenditure and vaporization loss. In order to deal expeditiously with the conversion phenomenon, he must know the rate of conversion of ortho to para hydrogen over various catalysts for many different conditions of temperature and gas pressure. He must also assure himself of a reasonable lifetime of the catalyst under the conditions that are to prevail. What of the effects of traces of impurities such as water vapor or air? Are the physical properties of the catalyst suitable? And the heat transfer properties? All these and other, similar questions can be answered only by engineering research. There is no theoretical structure that can be resorted to for this kind of information; nor are there handbooks to refer to in view of the fact that ortho-para conversion of hydrogen was only a scientific curiosity a scant eighteen months ago. Engineering handbooks are compilations of the results of many years of engineering research. The researcher of today is writing the handbooks of tomorrow.

The designer of a new piece of equipment is fortunate indeed if he can find all he needs to know in existing handbooks. Even in the more classical areas of engineering research, such as strength of materials, heat transfer, fluid flow, and wave propagation, the information is sketchy at best. With increasing frequency, as design problems become more complex, the engineer must resort to experimentation to extend the available information or test the accuracy or sufficiency of some essential bit of scientific data for use in the problem at hand. From this discussion it may appear that engineering research is just a special example of applied research. I would take no exception to this conclusion. Where the scientist has new knowledge and understanding of natural phenomena as the primary objective, the engineer is concerned with the creation of a "thing" which in the perfection of detail can be built repetitively from commercially available materials with existing skills for use and maintenance. The automobile is one of his great monuments, as measured by these standards.

Our early industrial progress depended largely upon the genius of lone inventors. These men, by present-day standards, were often neither scientists nor engineers. Their efforts were inspired by intuition and supported by the rare qualities of patience and persistence. Their methods were empirical—trial and error—and their authority came from within themselves. They were in many instances the prophets of things to come; in the nineteenth century their practical achievements laid the firm foundations of our major industries.

Leonardo da Vinci anticipated in the fifteenth century many of our modern devices—the machine gun, the aerial bomb, the tank, the helicopter, the parachute, and double hulls for ships. The first sketches of such devices as the flying spindle, the circular pulley system, a differential gear, the jack, and the water turbine were made by da Vinci. His speculations, although reflecting understanding of the basic principles of mechanics, were not embodied in useful devices for several centuries.

The two Stanley brothers, one of whom I was privileged to know as a neighbor, were successful inventors in several fields. To photography they contributed the first dry plate. They might well have conceived of Kodachrome or Technicolor, but I doubt very much if they could have developed these into acceptable use. Their steam-driven automobile was a great achievement, but a turbo-jet would have been beyond their reach. Such statements do not, however, deny inventive genius a vital place in the changing scene, as attested by the number of patent applications filed each year in the major fields of industrial research and engineering. The organized effort that has succeeded to the role of the Edisons and the Westinghouses makes good use of inventive talents. The fundamental scientist must often rely upon intuition for a first glance into the unknown, and use the empirical methods of the inventor where scientific understanding is for the moment lacking.

Engineering research has thus largely replaced the inventor, but his talents have been absorbed, not discarded. The increasing complexity of engineering goals demands something more than intuition and inventive inspiration.

The new working alliance between the scientist and the engineer has found one of its finest and most fruitful expressions in the development of scientific tools. A trip through our postwar laboratories—industrial, government, or university—cannot fail to impress upon anyone the greatly increased dependence of investigation upon instruments. The newer instruments of physical research were previously cobbled and put together in a makeshift way. There is now very much in evidence apparatus that has been highly engineered, with meticulous attention to details of design, making for reliability in measurements and for use by other than the most highly skilled scientist. *Fortune* magazine in its December 1952 issue notes that there are today about thirty United States firms turning out a dozen or more laboratory mechanisms that cost over $10,000. Supplying our research laboratories with engineered scientific tools has become big business. The *Fortune* article notes four of these as being in the forefront of this new era: X-ray diffraction equipment, the mass spectrometer, the ultracentrifuge, and the Collins helium cryostat. These instruments are playing a most important role in the new methodology of engineering research, of which they themselves are the product, as the tools of both the scientist and the engineer: they speak the language of both, which is numbers.

With improved communication, all barriers are breaking down. Creative technology is today largely dependent for its achievements upon a hybrid, the scientist-engineer. Which talent of this dual personality comes first in title depends on whether the individual is at a given time oriented toward the "think" or the "thing."

In this new era of creative technology, the engineer has become increasingly dependent upon the scientist, whose progress he no longer follows at a distance, but with the intimacy of the next-door neighbor. At the frontier of every art, the engineer is restricted by lack of understanding, data, or material. He may clearly perceive the direction in which improvements may be achieved or revolutionary developments accomplished, but before he can pioneer, the scientist must explore and achieve understanding. The scientist, in turn, is increasingly dependent upon the engineer for the design and construction of essential tools and for the challenge to push forward the frontiers of knowledge. This interdependence is significant for the future of both scientific discoveries and engineering progress.

Soil, Animal, and Plant Relations of the Grassland, Historically Reconsidered

JAMES C. MALIN

Dr. Malin's ecological studies have appeared several times in THE SCIENTIFIC MONTHLY, *but in his present article he appears in his proper guise as a historian. His discussion is based on a paper presented before a joint session of AAAS Section G, the Ecological Society of America, and the Grassland Research Foundation in a symposium on "The Western Range," which was held during the 1952 Annual AAAS Meeting in St. Louis. Dr. Malin is a professor of history in the University of Kansas.*

I. The Problem of Method and Point of View

WHEN a historian appears on a scientific program, it may be appropriate to ask some questions. What is science? What is history? The answer to these questions is not necessarily difficult. In dealing with the field sciences, as distinguished from the laboratory sciences, history and science may be only different facets of the same thing. Nowhere is this fact more relevant than when applied to any consideration of natural resources.

But, again, a question. What is a natural resource? The answer is that the properties of the earth become natural resources only as they involve man and are utilized by him. Without the intervention of man, although particular properties are actually present, they are latent, or unrealized. A natural resource is not determined by the properties of the earth, per se, but by the qualities of the mind of men. The first requisite of a natural resource is an idea. There are no known limits, therefore, to the multiplication of natural resources of the earth, and exhaustion of them is impossible, except, or unless, the capacities of man are exhausted—the capacities through which the latent properties of the earth are discovered and thus become properties new to man and available to his use as natural resources.[1] The record of the process by which the potential of the earth has been made actual is within the province of history, and to the study of it the historical method should apply, regardless of whether the intellectual enterprise is undertaken by the historian or by the scientist. In this context, history provides the background and prepares the setting, but at that point science may take over. Obviously, the boundary lines that have become traditional between the several accepted intellectual disciplines are artificial, and as they were adopted originally only for the purpose of making the intellect more effective through specialization, they are justified only so long as they accomplish, rather than hinder, that purpose. To achieve the goal of making the mind most effective, intellectual enterprise must possess both perspective and depth, and to sacrifice either defeats the full realization of the other. History is of particular importance in establishing perspective.

When the problem of history and historical method are introduced, the time has come to distinguish two schools of thought on the subject. First, the concept of objective history strives to reconstruct historical actuality as completely as possible, without respect to any possible use to which it might be put. Second, the subjective relativist functional notion about history holds that the only excuse for its practice is to make it useful for some present purpose. To accomplish this functional goal a selection from the whole corpus of historical actuality is made, utilizing only the so-called usable part. The great difficulty with this method is that the results are almost certain to be predetermined by the frame of reference adopted before the so-called historical investigation was begun, only those things being found, or considered applicable, that fitted the preconceived hypothesis.

The requirement of usefulness more often than not defeats itself. On the contrary, the first method described, the pursuit of knowledge as intellectual enterprise, without respect to usefulness, offers more probability of turning up something useful, although not necessarily anything that was in the mind of the investigator when he began.

II. Illustration: Roe, *North American Buffalo*

In order to reduce the problem to something tangible, the book by Frank Gilbert Roe, *The North American Buffalo: A Critical Study of the Species in its Wild State* (1951), is taken as a remarkable example of excellent history that is at the same time essential to the scientist and to the historian in more ways than the book reviewers have thus far recognized. All reviews of the book that have come to the notice of the present writer have been quite favorable, but in spite of that fact the impression conveyed is primarily that it is just a good book that should be read sometime. Certainly, without the intent of the reviewers, that is almost equivalent to damning it with faint praise; because Roe's *North American Buffalo* is a book that is of such outstanding importance that anyone interested in the grassland of North America, whether he be historian or scientist, should read it immediately, the whole of it; not only read it once, but reread it and digest the contents thoroughly. This book should be read not only for what is actually said, but it should be studied in all its implications in order to search out all the possible ramifications. Studied with thoroughness, it should become the springboard for a wide variety of investigations in several disciplines not even contemplated by its author.

Frank G. Roe is a resident of Canada, and he is not a professional historian. He became interested in the buffalo problem as a by-product of a study of the earliest roads in old England and arrived at the unorthodox conclusion that they "were probably *not* originally wild animal tracks; nor were the earliest human (Indian) trails of this continent [North America] buffalo tracks." The contradictory historical evidence relating to the buffalo led him into a fifteen-year study of the buffalo, with special reference to the part of the North American continent, north of approximately 40° north latitude. He devoted only limited attention to the country south of the Republican River. Another self-imposed limitation was to exclude scientific considerations, but no scientist should be misled into assuming that the book has no value for scientists. The facts are quite otherwise. Roe's statement of reasons is fundamental:

> In dealing with an animal now extinct as a free world species in its most characteristic native habitat, the first task is to ascertain and classify the historical evidence; and not until this has been done can biological investigation proceed with much profit.

The evidence collected appears to support the conclusion that some variation did exist within the buffalo species. In relation to the long-accepted tradition that the buffalo made general annual migrations from the south to the north and return, Roe has demonstrated conclusively that no such general movement occurred, especially north of the Republican River country, or about the 40th parallel. He concedes that some such movement occurred south of that boundary, but, as a part of the self-imposed limitation of the geographical scope of his study, he did not undertake to survey southern literature intensively.

Roe's contention, that the buffalo movements were primarily random wanderings, appears to be fully demonstrated. The Indians who were largely dependent upon the buffalo followed these random wanderings as best they could. Thus the numbers of buffalo that might occupy a particular spot could be enormous, and damage to vegetation disastrous, but the incidence was not continuous. There is still an opportunity for other investigators to study thoroughly the problem of overgrazing, drought, and dust storms under aboriginal culture.

Although Roe did not go into the problem of the consequences entailed by his conclusions, it is in this context that we learn that surface erosion by wind and water was present and upon occasion severe under aboriginal conditions—recurring drought, fires, overgrazing, and trampling by animals, especially buffalo, as well as the wearing of innumerable paths to watering places. Dust storms upon a large scale were not caused by the "plow that broke the plains."[2]

Roe has demonstrated that the migrations of the buffalo were primarily random. But there are other aspects of his treatment of buffalo movements that are not so satisfactory. He challenged the notion that buffalo changed their direction of movement deliberately on account of encountering sparse grazing, and that they sought out more productive pastures over considerable distances. Yet he accepted the view that buffalo moved between the plains and the Rocky Mountains in midlatitudes, and between the rough wooded areas and the plains in the north country. Probably he is correct in the sense that he ruled out in the first case the notion of buffalo capacity to make choices on the basis of memory or instinct akin to rationalizing from experience, but that does not explain

the behavior which he does seem to accept, that of seeking shelter in timbered areas during the winter storms or for shade from the intense heat of the sun. A suggestion is offered here that possibly guidance may be derived from the physicists' theory of the unpredictable behavior of individual particles, but the high degree of predictability in the sense of statistical probability as applied to behavior of large numbers.

Although Roe emphasized the fact that he was not a scientist and was deliberately excluding scientific aspects from his book, he did not escape making scientific blunders. One of these may be mentioned as illustrative of the importance of the historian's knowing something about science. In discussing the extensive deposits of buffalo bones scattered over the plains, which must "have been broken, crushed and stamped into the earth," he suggested that: "This also may have some bearing on the enrichment of the soil. A chemical analysis of Kansas virgin prairie soils might yield some interesting information" (p. 515, Note 116). He was unaware of the epoch-making monograph of E. W. Hilgard, as long ago as 1892, which demonstrated conclusively the fact of lime accumulation in soil of low rainfall climates, regardless of the parent materials from which they were derived.[3] Such additions to the lime content of the soil as buffalo bones or any other artificial additions of lime to lime-rich soil contributed nothing to soil properties or to productivity.

In reviewing the literature dealing with the buffalo, Roe has demonstrated that the most recent is not necessarily the best. Joel A. Allen's book, *The American Bisons, Living and Extinct* (1876), is among the earliest formal treatments and was the best of the lot. Roe demonstrated also that the scientist is not necessarily the best authority, both Hornaday and Seton being proved quite unreliable except upon limited aspects of the subject. George Catlin, an artist, emerged conspicuously as one of the most reliable observers of buffalo and Indian lore. Furthermore, Roe's study made embarrassingly clear that the medium through which a supposedly scholarly study is published is not necessarily an index of its authenticity. Again, Hornaday's monograph, the work of a scientist, published by the Smithsonian Institution, is the horrible example. This mid-twentieth-century culture is the victim of a naïve worship of formal training and specialization, forgetting too much the first principle, that competence in any field is grounded in the quality of the individual. Roe is not a professional historian, and he disavows explicitly any scientific pretensions, yet he has produced a major historical work that is fundamental to both historians and scientists. The only plea that may be appropriately advanced in this connection is that some formal discipline in history and science and their respective methodologies might have enabled Roe to produce a still better book. There is room for an argument in rebuttal, however, that the requirement of formal methodology and training might have killed all incentive to write the book.

As a result of Roe's study of the buffalo, both the historian and the scientist must largely rethink the whole problem of the interrelations of the buffalo and of man, and of many corollaries or inferences that are applicable to the grazing of domestic livestock on grass. One more point in emphasis from Roe may be permissible, one which is in a sense the major point of the present paper. Roe defined his idea of the relation of history to science, and with the qualifications given above, that declaration stands as the view of the present author:

> The first task is to ascertain and classify the historical evidence; and not until that has been done can biological investigation proceed with much profit.

III. Animal Exclusion Studies

Attention is now directed to another type of study—this one an experimental project conducted in the field. To facilitate objectivity, an English work is used, that of V. S. Summerhayes, "The Effect of Voles (*Microtus agrestis*) on Vegetation."[4] By exclusion of voles from the test plots over a period of seven years the conclusion was arrived at that the yield of dominant grasses was increased:

> On the removal of the vole attack the non-dominants, particularly the mosses, decreased in abundance, apparently as a result of the increased competition with the more luxuriant dominants. Voles therefore tend to preserve a relatively open vegetation, comparatively rich in species ["flowering plants and especially mosses"]. This is presumably effected by the direct eating or cutting up of the aerial parts of the dominants, and by the complicated series of burrows below the main surface of the vegetation, the formation and maintenance of these burrows preventing the development of large tussocks of grasses like *Molinia*, or thick-matted turf-like growth as in *Holcus Mollis* (p. 45).

The author let the matter rest with those conclusions, and refrained from any policy recommendations, but the customary policy conclusions drawn from such animal exclusion studies are that the predators should be exterminated to increase the grass yield available for livestock. Such policy conclusions do not necessarily follow. The duration of the experiment was seven years, but what might have been the result if it could have been continued

one hundred years? The central point is that soil as an object of study had no place in the experiment, yet any assumption about indefinite maintenance of the increase in yield of dominant grasses must be posited upon a parallel assumption of an indefinite maintenance of soil productivity. Questions that require answers on such a long-term basis include the status in the investigation of legumes and of deep-rooted forbs, and of the activities of animals and of the soil population, all considered in relation to the soil as an object of study. Had soil been included within the scope of the project under consideration, the seven years of effort might have meant being seven years further along on the study of the changes that occur in soil under the conditions of the experiment. What has just been said about the particular project under consideration applies substantially to similar work in the United States.

IV. Man within the Ecosystem

The process of the expansion of European culture throughout the world, a four- to five-century drive that has about spent itself, was characterized conspicuously by a contempt for the "savage" and the "backward" peoples of the globe. No branch of that culture was more conspicuous in that respect than the Anglo-American tradition. Belatedly, the situation is changing, during the mid-twentieth century in particular, and re-examination of old evidence and discovery of new facts are revealing fresh perspectives which impart to aboriginal culture a historical significance of outstanding importance. The conventional or traditional concept of the state of nature must be abandoned —that mythical, idealized condition, in which natural forces, biological and physical, were supposed to exist in a state of virtual equilibrium, undisturbed by man. The role of aboriginal man within the ecosystem must be recognized as a major ecological fact. The task of re-examination, largely historical in character, cannot be done in a day, and it has not been done for the North American grassland.

V. The Great American Desert: Semantic Problems, Myths, and Legends

In dealing with the North American Grassland historically, one of the first problems to be met is that of the semantics of the word "desert." Approached from the standpoint of the history of the usage of the word, many of the difficulties are revealed. The meanings varied widely in time, and otherwise. In the eighteenth century, and even during the early nineteenth century, good usage included the idea of an area that was deserted—especially deserted by man—therefore, desert, even though it was covered with forest. The word did not necessarily have reference to consequences of the relations of climate to vegetational cover.[5] Some forest men used the word in such a manner as to imply that a lack of trees and running water made a desert, even though a grass cover was present.[6] For the accurate interpretation of written documentary evidence, therefore, the necessity arises of determining what the original observer meant by desert, as well as the concept that existed in the mind of the person using the document.

A major myth developed during the nineteenth century, that a Great American Desert stretched across much of the western interior of the continent, and the label was placed upon some maps. That fact led to another legend that the myth of the Great American Desert was held universally, but of course that was not the case. At no time were either the literature or the maps in general agreement on the existence of a great desert or of its extent. In the monumental *Atlas of Historical Geography of the United States* (1932), edited by C. O. Paullin, ten maps were selected to illustrate the development of the cartography of the western United States, bearing dates from 1804 to 1867. Only two of these used the "Desert" label. Pioneers, eager to occupy the land, were optimistic about the possibilities of the country, as were promoters of railways to the Pacific coast, unless describing the route of a rival.

The gold rushes of 1849–59 contributed substantially to public education in the geography of the West. R. T. VanHorn, editor of the *Journal of Commerce*, Kansas (City), Missouri, on November 10, 1859, commented optimistically that the desert of the myth had retreated from Illinois westward and the gold rush of 1859 had finally extinguished it, except the Senatorial Desert, which existed only in the Senatorial Mind at Washington.[7]

To put the question more broadly, there was a general tendency for those who opposed the rapid settlement and development of the trans-Mississippi West to be receptive to the desert myth, whereas those favorable to the aggressive westward expansion were sure and determined that all that was necessary to make the grassland blossom like the rose was to let in the population. In an able editorial in the St. Joseph *Gazette*, June 14, 1854, Lucian J. Eastin reviewed explicitly this conflict in outlook, and the reversal in point of view after the annexation of the Southwest, the opening of emigrant roads, the gold rushes, and the establishment of trade routes.

VI. The Problem of Origin of the Grassland and Climate Change

Closely related to the desert myth problem is that of the "origin of the prairie." The single point dealt with here is the factor of fire, whether natural, accidental, or used deliberately by the aboriginal population. The notion of the fire origin of the grassland may be dismissed, but in transitional country, so far as climatic and local factors were concerned, fire did act generally to restrict tree growth. Recognizing that fact, some important historical conclusions are in order. During the years following the Civil War, the idea became widespread that the climate was becoming more favorable as a result of settlement. That interpretation was a quite reasonable one, if viewed against the background just indicated. Settlement eliminated fires, and woody growth spread at the expense of grass.* The assumption became easy that this would continue until the whole area would support tree growth, if permitted. The fact of the surprising extent of the spread of trees was inescapable, but the interpretation of the facts in terms of climate change was erroneous. The white occupants of the grassland did not understand the role of the multiple factors in the situation that had operated under aboriginal culture; hence, the misinterpretation of causal relations.†

VII. Soil as an Object of Study

The introduction into this discussion of the subject of soil as an object of study is a sharp reminder that the literature from which the history of the soil conditions under aboriginal culture and European man's attitudes toward them is as contradictory as the buffalo literature with which Roe dealt. Any clear and reliable understanding of the soil problem in its essential historical aspects awaits comprehensive historical treatments on a comparable scale.

Some accounts emphasize the hardness and impervious character of the soil as found under aboriginal culture, a compactness so repellent to water that the rainfall ran off into the streams, producing floods or severe erosion cutting deep channels. Thus in the upper Canadian River, about 103° west longitude, Lieut. J. W. Abert commented, in 1845, that the prairie, "baked in the hot sun, absorbs but little water. . . ."[8] Other accounts stressed the soft, yielding character of grassland geological structure, and the rapidity of erosion, by water and wind, of the unstable soils.[9]

Many of the early travelers and explorers were impressed by the activities of such burrowing animals as ants, pocket gophers, ground squirrels, and prairie dogs, and the activities of buffalo disruption of stabilized soil conditions. In 1846, on a military mission in the opening months of the Mexican War, Lieut. Abert commented on the activities of pocket gophers along the Santa Fe Trail between 96° and 97° west longitude. His journal entry for June 28 reads:

> Whenever we rode to the side of the road we noticed that our horses would frequently sink to the fetlock, and saw on the ground little piles of loose earth . . . formed by the sand rats, or gophers. . . . [Four days later he added]: The mounds of the gophers . . . were more abundant than heretofore, and in several places a number of these mounds had been so close together that the distinctness of each was completely lost in the mass, covering an area of five or six feet.[10]

This description applied to tall grass prairie, but Abert commented later on pocket gophers in the Arkansas Great Bend area. All the country in question was in the condition commonly defined as "virgin prairie," or "in the state of nature."

In the narrative compiled by the botanist Edwin James for the Stephen H. Long expedition of 1820, descriptions are given of extensive prairie dog towns in what is now Nebraska and Colorado.[11] Among the most detailed descriptions of prairie dog towns are those of Captain R. B. Marcy, covering exploring expeditions on the southern Great Plains in 1849, 1852, and 1854, especially the Red River report of 1852 which included the valley of the South Fork, a stream which the Comanches called Prairie Dog Town River.

The contradictory character of the literature, both historical and scientific, on these problems seems to call for comprehensive investigations of both types, not only of the animals, but also of soil, as an object of study under the influence of these animals, and after they have been eradicated. Sites that are known to have been occupied in Nebraska and Colorado in 1820 or in Texas in 1852 might profitably be studied to determine what influence such occupation imposed upon the soil. Sites might be selected where the date of eradication can be established, to determine what has happened to such soils without the presence of prairie dogs, or of other burrowing animals that

* In his book, *Colorado, A Summer Trip* (New York, 1867), Bayard Taylor gave a vivid description of the landscape in transition in eastern Kansas and eastern Nebraska. It is a significant document so far as it was descriptive of what he observed.

† In Book Three of his book *Virgin Land* (Cambridge, Mass.: Harvard Univ. Press [1950]) Henry Nash Smith has performed the most complete job thus far in confusing the problems of the desert and of climate change, along with land policies.

may properly be studied in the same fashion. Such studies as are suggested here require as a preliminary step the same type of comprehensive historical study that Roe gave to the buffalo. Even when it is conceded that soil is benefited by such animal activities, there is no agreement upon what degree of disturbance by animals is advantageous to long-term equilibrium.

In pursuing the ecological literature about the grassland another gap is conspicuous—the function of deep-rooted plants of the nonleguminous families. To be sure, there are many studies of roots, and noteworthy are those of J. E. Weaver and associates, but they are oriented from the standpoint of plant ecology, not of soil science. The literature of the explorers contains many references to the range and distribution of such plants, which stand as a challenge to the historically minded to investigate certain of them comprehensively in relation to soil as an object of study. An example that invites investigation is the man-root, a morning glory, *Ipomea leptophylla* (Torr.), found, according to Gray's *Manual of Botany* (1889), on the "plains of Nebraska to central Kansas, Texas and westward." It produced a root the weight of which was given as ranging from 10 to 100 pounds. Lieut. Abert described his experience with it in 1846 while waiting for high water of the Pawnee Fork to subside, in the general vicinity of Larned, Kansas, about 99° west longitude, in the hard land north of the Arkansas River.[12] A soldier spent several hours trying to dig up a specimen under Abert's direction, but the ground was so hard they finally gave up and broke it off. The stem, about half an inch in diameter, ran down about 12 inches, then enlarged suddenly to 21 inches in circumference, or about 6 inches in diameter, and extended about 2 feet deeper. Abert's comment indicated that this specimen was relatively small compared with others supposed to grow to the size of a man.

From the standpoint of soil as an object of study, what happens to soil when a root expands to 6 inches or more in diameter, displacing the soil to a depth of three feet or more? When the plant died, the root decayed, and the cavity was refilled—but how, and how rapidly, and with what effect on the soil? What was the actual floristic range and the density of distribution of this plant, and its average and maximum life expectancy? There were many other grassland forbs, with roots of smaller diameter, that penetrated the soil 10 feet or more. All these deep-rooted plants penetrated the lime accumulation zone. Abert commented that the Cheyenne Indians dug and ate the man-root. If they did it generally, they must have possessed more patience than Abert and his soldier, because the Indian had no iron tools with which to dig. Also, such digging substantially disturbed the soil.

Soil should be investigated as an object of study under aboriginal conditions as a prerequisite of scientific investigations carried out under existing conditions, or artificially controlled conditions. Such historical investigations should recognize all possible factors: aboriginal man; large animals; the smaller animals, especially the burrowing animals; insects that bury themselves in the soil; the deep-rooted plants.

Again, the *Ipomea leptophylla* may be used as an example in order to make the discussion more concrete, although prairie dogs, or pocket go... or ground squirrels might serve as well. ... ering the extent of vegetational distribution qua... titatively, this plant was engaged in a continuous soil tillage operation. New plants replaced old ones; new growth in one spot displaced the soil, while decay of old roots at another permitted the cavity formed in the soil to collapse. But from what directions: From top down, or did the sides cave in? Or both, on occasion? The vital issue is that the tillage was continuous, but without de... stroying the soil cover as mechanical tools tend to do, and it was to varying depths; possibly, where subsurface conditions permitted, the prevailing depths were 2–4 feet. Lesser roots penetrated much deeper. No mechanical tool has been devised for cultivation of the soil that can perform a comparable job, that can open up the soil body to a depth of 30 inches or more, and certainly none that can open up the soil to any considerable depth without destroying the vegetational cover. To what extent did these processes interrupt or modify theoretical profile-forming tendencies and the lime-accumulation zone? What happens when these factors are removed altogether by eradication programs or clear-field cultivation?

From this historical approach to the problems of the grassland the conclusion has been reached that erosion, in the much-advertised sense, is not necessarily the most important aspect of soil conservation. In any case, the critical aspects of soil conservation vary with particular spots. They depend upon time and space. But, in many respects, more fundamental than the several facets of surface erosion is more knowledge about soil in a comprehensive sense under aboriginal culture, and what happens to soil internally as a consequence of the transition from aboriginal occupation to utilization by modern society—in the transition

from natural tillage by wild animals grazing, by burrowing animals and insects, and by the influence of the native legumes and deep-rooted forbs to the twentieth-century mechanized regime.

VIII. Water Table

One contention of the present author, not yet given a full-scale demonstration, is that availability of well water for livestock and domestic purposes played a role in settlement survival during the pioneer period that may have been even more decisive than rainfall for grass and field crops. The drought decade of the 1930s focused attention upon water supplies for cities, and set geologists to work with a new vigor upon Pleistocene geology.[13] Among the results of such research is the conclusion that the water table generally ssentially stable, varying temporarily with ic fluctuations and local circumstances. Soil science placed a new emphasis upon the water table, assigning to it a role as a soil-forming factor.[14] Thus, by a rapid succession of events, the original proposal, the study of the relation of well water to pioneer settlement in the grassland, has een given a more fundamental significance extending into the far broader issue of the water ble in relation to the whole problem of human occupance of the area. The subject is so large as to offer research opportunities for a number of students equipped for the task.

IX. Mesquite

Another aspect of the problem of man within the ecosystem may be illustrated by reference to an article "Man vs. Mesquite," in *Life* magazine, August 18, 1952. The caption under the map read: "Mesquite march during last 100 years has taken it from small riverside areas in which it grew in 1850 to the 75 million acres it now covers . . ." (p. 69). Inquiry concerning the authority for the map brought the answer from the editors that

> A century and a half ago, there was hardly any [mesquite] in the U. S., but during the next fifty years it was brought into this country from Mexico by Spanish ponies and by wandering herds of wild buffalo. So that, in 1850 (as shown by the map) scattered stands of mesquite were growing along the creeks and river beds of the southwest. This first generation mesquite, however, was exceedingly sparse . . . during the great cattle drives of the second half of the nineteenth century (roughly 1860 to 1880), the cattle intensified the mesquite in the areas along the watercourses, and extended it out onto the plains away from the creeks and streams, and since then mesquite has fortified its hold on the southwest to cover the area shown in the 1952 distribution on the map. . . .[15]

Another statement on the mesquite history is that in the Clements-Shelford textbook *Bio-ecology* (1939), where mesquite in the costal and mixed prairies was attributed to a disclimax induced by overgrazing which took the form of "a savanna of mesquite and cactus" (p. 279). And in still another place, V. E. Shelford declared:

> For example, the cattle business of the United States had its beginning in the gulf coast tallgrass prairie. This is an area almost universally mapped as mesquite—chaparral or savannah and regarded by many as having been that type before the white man came to the area. On the contrary, since cattle eat the mesquite beans and fail to digest them, they spread the seed widely and may be responsible for the entire savannah. It is well known that the mesquite has been spread from south central Texas into west central Oklahoma by this method.[16]

Some historical data may now be brought to bear upon the mesquite problem in order to establish some factual landmarks. The Stephen H. Long expedition of 1820 found mesquite in the Canadian River country, and Edwin James, who prepared the report of the expedition is credited with the first public notice of the mesquite tree.‡ Lieut. Abert found "an abundance of mesquite" in 1854, growing about 103° west longitude as a shrub about 5 feet in height in what is now northeastern New Mexico above the headwaters of the Canadian River.[17] The R. B. Marcy expedition up the Canadian River, in 1849, found mesquite just east of the Llano Estacado escarpment, the journal entry stating that "We found a great deal of the small mesquite . . . today."

Marcy's return route from Santa Fe, in 1849, turned southward down the Rio Grande to Doña Ana, thence eastward to the Pecos River down that stream to the crossing; then, skirting the escarpment of the Llano Estacado, he struck northeastward across Texas. The mesquite was brushlike in the country west of the Pecos, but increased to small tree size at that river, and eastward as the ascent was made into the high plain and in the Big Spring area it attained large tree size. From the latter point northeastward, Marcy's map indicated mesquite timber. The second day after Big Spring, the route led "over rolling and rather broken country, of good soil, and covered on each side with large mesquite trees." Near what he miscalled the Double Mountain Fork of the Brazos River, Marcy recorded: "We have been travelling through groves of mesquite timber, with a beautiful carpet of grama grass underneath, nearly all day." On the next day, on the south side of Double Mountain Fork, he continued over

‡ R. B. Marcy recognized this fact in his report on the Brazos River expedition of 1854, quoting from John Torrey, by whom the mesquite species collected by James was described and named *Prosopis glandulosa*.

as beautiful a country for eight miles as I ever beheld. It was a perfectly level grassy glade, and covered with a growth of large mesquite trees at uniform distances, standing with great regularity, and presenting more the appearance of an immense peach orchard than a wilderness. [Heading toward the Brazos River above the mouth of the Clear Fork] The mesquite wood and grass continued very abundant. . . .

Four days later, just before crossing the divide into the watershed of the West Fork of the Trinity River and west of the 98th meridian, mesquite and oak openings were reported, with occasional prairies. In summing up the estimate of his line of march as a route for the Pacific Railroad, Marcy reported 200 miles "over a gently undulating country, with prairies and timber," springs and streams, "in many places covered with large groves of mesquite timber, which makes the very best fuel," and later he made a more positive commitment to the existence of "an inexhaustible amount of mesquite timber, which, for its durability, is admirably adapted for use as sleepers, and for fuel."[18]

In 1852, Marcy explored the headwaters of the Red River. When west of the 101st meridian on the South Fork of that stream he wrote:

We find much more mesquite timber upon this branch of the river than upon the other. Indeed, I have never seen much of this wood above the thirty-sixth degree of north latitude; but south of this it appears to increase in quantity and size as far as the twenty-eighth degree. Upon the Canadian river I have observed a few small bushes; but the climate in that latitude appears too cold for it to flourish well.

In the same report, in his discussion of the Pacific Railroad by the southwest route of his exploration of 1849, Marcy wrote that after crossing the Brazos

the road skirts small affluents of that stream and the Colorado for two hundred miles. . . . Here and there prairies present themselves, but this section is for the most part covered with a growth of trees called mesquite, which stand at such intervals that they present much the appearance of an immense peach orchard. They are from five to ten inches in diameter, their stocks about ten feet in length, and for their durable properties are admirably adapted for railway ties, and would furnish an inexhaustible amount of the very best fuel. . . .[19]

In 1854 Marcy explored the headwaters of the Brazos and Big Wichita rivers. From Fort Belknap, heading west of north, they passed over "rolling country, covered with groves of mesquite trees." The next day they crossed tributaries of Trinity River, "all of which were wooded with mesquite, and occasionally a grove of post oak seen, with here and there a cotton-wood or willow tree along the banks." Later, "On leaving the Wichita, we travelled south towards the Brazos for six miles through mesquite groves. . . ." From a low mountain near the Brazos, Marcy described the scene: "Towards the east from this elevation nothing could be seen but one continuous mesquite flat, dotted here and there with small patches of open prairie, . . ." and on the next day: "The country we are now passing is gently undulating and covered with mesquite trees."

By the time Marcy made this expedition he was much impressed by the mesquite and wrote a rather comprehensive summary of the subject some three to four pages in length:

In the journeys I had made before upon the plains, I had observed the mesquite tree extending over vast tracts of country, and I had noticed some of its useful properties, such as its durability and its adaptation for fuel, but I was never so fully impressed with its many valuable qualities as during the past summer.

It covered a great portion of the country over which we travelled. . . .

It was at this point that Marcy acknowledged that Edwin James, of the Long expedition of 1820, had given mesquite the first public notice. In commentary upon the range of distribution, Marcy admitted limitations of information, but east of the Rocky Mountains he defined its limits as between 97° and 103° west longitude, and between 28° and 36° north latitude; but west of the Rio Grande the mesquite flourished best in the valley of the Gila River. In the plains, however, he remarked that the size diminished north of 33° and to mere bushes at its northern range limits of 36° north latitude.§ In its tree form, it ranged in size from 4 to 15 inches, and was not more than 20 feet in height, and furthermore was "much used for building in southern Texas and Mexico," being well preserved in the ruins of old buildings. And then Marcy recorded information critical to the ecological problem, reporting that mesquite often grew "upon the most elevated arid prairies, far from watercourses," but it would grow only upon good soil, and that settlers competed for mesquite land.[20]

A second account of the Marcy Brazos exploration is available in the book of W. B. Parker, a civilian. In many respects Parker's version is similar to Marcy's, but variation in presentation of the scenery affords some further enlargement of perspective. Between the Cross Timbers and the Little Wichita, on July 11, the entry read: "The country we had been passing over, since leaving the Cross Timbers, was a rolling prairie, very thin

§ Marcy did not leave any account of having explored north of that limit, so he was indicating in part at least only the limits of his firsthand knowledge.

in soil and timber very scarce. At this point we began to find the mesquite trees in great abundance." Their size was given as 4–15 inches in diameter and not more than 20 feet tall. These were the same specifications as those given by Marcy. In addition to its qualities as fuel (burning like hickory wood), Parker added, "and not the least is its durability for building purposes—. . . invaluable to the future settlers."

An entry five days later recorded "ascending in a northwest course, a rolling country, covered with buffalo grass and mesquite timber. . . ."‖ Three days later, approaching the Little Wichita, a belt of timber marked its course, "and in front the wide prairie with its yellow coating of buffalo grass, studded with the pale green mesquite, a beautiful combination for a landscape painting." Again, a few days later: "Our course was Northwest, and ascending gradually, we came upon a very extensive plain, covered with buffalo grass and mesquite timber." Later on, a course south from the junction of the three prongs of the Little Wichita, they "entered an extensive plain covered with thin coarse grass and stunted mesquite timber." Upon arriving at a spur of the Llano Estacado, they ascended it "to a broad level plain . . . covered with buffalo grass and mesquite trees, and extending as far as the eye could reach in a perfect level toward the dim cloud like mountains at the head of the Brazos." Arriving at the escarpment of the Llano Estacado August 3, they climbed to the top, and looking eastward from an elevation estimated at 600 feet above the country below:

> The view was the most extensive and glowing in the sunset, the most striking that we had enjoyed during the whole trip, combining the grandeur of immense space—the plain extending to the horizon on every side from our point of view—with the beauty of the contrast between the golden carpet of buffalo grass and the pale green of the mesquite tree dotting its surface.[21]

Admittedly, the foregoing survey does not cover anything like all the literature, and certainly there is no intent to exaggerate the extent of mesquite occurrence, but it makes abundantly clear the fallacies widely held about the mesquite problem, especially those in evangelical conservation circles. In order to bring this discussion to a focus, a few tentative conclusions are outlined, derived from the limited historical data cited from the reports of the Long and the Marcy explorations. First, in a floristic sense, the geographical range of distribution of mesquite (*Prosopis* spp.) is about the same in 1952 as at the opening of the nineteenth century, or 150 years ago. Possible extensions of floristic range appear to be a minor aspect of the problem. Second, in a vegetational sense, the quantity of mesquite at the midpoint of the nineteenth century was substantial, and was not limited to the banks of streams; upon occasion mesquite occupied broad plains and rolling hills in west and north central Texas as far west as the Llano Estacado. Repeatedly the Marcy descriptions of the country indicated extensive reaches of mesquite savannah, with occasional patches of open prairie. Such language appeared so often, and so explicitly, as to be both significant and important. Third, in an ecological sense, the focus of interest is the change in the behavior, or growth form, of the mesquite during the century 1852–1952. As an ecological fact, the nature of mesquite occupance in much of the region under review changed from a savannah to a tangled jungle, in places almost if not quite impenetrable. The outstanding ecological problem, then, is to find an explanation of the how and the why of this change in growth form of mesquite and its associates. An accurate historical study of what has happened, establishing in fuller detail the facts of floristic range limits, quantity of vegetation, and form of growth, prior to the time the Indians handed the land over to the whites, may put the ecologist and the range manager in a position to attack the question.

A fourth and fundamental conclusion is the full acceptance, as of long standing, of the mesquite occupance of the floristic range just indicated. Marcy gave the size of mesquite as ranging from shrubs to 15 inches in diameter. This in itself is proof of long establishment. Further evidence of the long duration of mesquite occupance in southern Texas, the portion inhabited by European culture, was the reference to mesquite timber found in the ruins of old buildings. In 1884, V. Harvard compiled a growth-age table for mesquite: a trunk diameter of 7–8 inches, 30 years; 8–10 inches, 50 years; 10–12 inches, 75 years; and over 12 inches, more than 100 years old.[22] According to such a calculation, a diminishing rate of increase of diameter with age must admit, for a 15-inch diameter, a life span of 150–200 years or more. As of 1952, that would carry mesquite occupance of northwest central Texas back in time 250–300 years—possibly more, on the basis of the Marcy evidence. This does not take into consideration the possibility that earlier trees may have grown, died, and disappeared prior to those he was describing.

So far as the buffalo and other wild animals operated as a factor in scattering mesquite, they

‖ Clearing up of confusion in the nomenclature of grasses would require a separate study.

wandered over the whole area for centuries before 1800. The accounts of the Coronado and De Soto expeditions record buffalo in the area about 1540–41.

So far as domestic cattle drives or domestic overgrazing were factors, according to the census of 1880, neither operated generally in the country west of the 100th meridian prior to about 1879. The cattle drives northward during the 1850s followed a path just west of the Arkansas western boundary, many crossing the Missouri River below Kansas City. The cattle business in the plains proper awaited the breaking of the Comanche-Kiowa Indian Barrier.[23]

The savannah form of vegetation was found in other parts of North America when Europeans took over the land from the Indians, and studies of it elsewhere may be profitable to establish perspective. N. S. Shaler, by profession a geologist, but by avocation a historian of the Kentucky country, long ago attributed the prairie condition of much of the area east of the Mississippi River to fire, occurring naturally, by accident, or as an instrument used deliberately by the Indians. Shaler did not make the mistake of assigning to fire the whole responsibility. In some areas, especially westward as the rainfall diminished, climate was held to be decisive. But in the Kentucky country, what Shaler described as essentially a savannah stage was a preliminary step in the process of reducing a dominantly forest area into a prairie. He associated the Indian practice with the eastward migration of the buffalo sometime after the year A.D. 1000 and suggested that had European intervention been delayed another 500 years, the prairie might have been extended to the Alleghany Mountains. Although Shaler's is a rather extreme view, Roe's study of the buffalo gives support to the factual portion of Shaler's general contention.[24] Shaler's dating of the arrival of the buffalo was established by excavations he had made in 1868 around the salt springs at Big Bone Lick, Boone County, Kentucky. In succession from about glacial times toward the present, bone deposits accumulated, the modern buffalo species occupying the top position—in time, later than the Mound Builders, who were not acquainted with the buffalo.

In the state of Mississippi as of the late 1850s, E. W. Hilgard wrote of the country as received from the hands of the Indians:

> The herbaceous vegetation and undergrowth of the Longleaf Pine Region is hardly less characteristic than the timber. Whenever the regular burning of the woods, such as practiced by the Indians, has not been superseded by the irregular and wasteful practice of the later settlers, the pine forest is almost destitute of shrubby undergrowth, and during the growing season appears like a park, where long grass is often very beautifully interspersed with brilliantly tinted flowers (p. 349).

The same writer, at another place, continued the theme under the head of "Pasturage in the Pine Woods":

> In their natural state, as received from the hands of the Indians, the Pine Woods were one great pasture—as, in thinly settled regions, they still are. Nor is it, generally, the ranging of cattle which has destroyed the pasturage in other regions, but simply the injudicious burning of the woods, at seasons when the fire would destroy not only the dry leaves, but also parch the *heart* and the *roots* of the grasses. It would seem that in a region comparatively poor in agricultural resources, the maintenance of pasturage should be considered a matter of national importance. The Swiss, being unable to cultivate profitably their mountain slopes, have converted them into pastures, these form the basis of their national wealth. Why this should not be so with the inhabitants of the Pine Woods, I have been unable to discover, it is certain, however, that the pasturage of that region, is disappearing before the fires at a fearful rate, and that those who heretofore have relied on the range, during all but a few weeks in winter, for the support of their cattle, will soon be compelled, as many are now, to raise feed for them on their poor soil, which, at present, will but just furnish comfortably the prime necessities of life for the population itself. The beautiful park-like slopes of the Pine Hills are being converted into smoking desert of pine trunks, on whose blackened soil the cattle seek more vainly every year, the few scattered, sickly blades of grass, whose roots the fire has not killed.

The preceding paragraph was descriptive of past and present. Hilgard then discussed policy and procedures in terms of management:

> It is not the province of this Report to suggest municipal regulations by which the burning of the woods at improper seasons might be prevented, or at least, rendered of less general occurrence; the evil, however, is a crying one to the mind of every candid observer, and the destruction of national wealth caused by it is so enormous as to deserve no less attention certainly, than the improvement of soils. However convenient and effectual may be the burning of the dry grass in order to render the young growth accessible to cattle, that advantage is certainly purchased very dearly at the cost of its total destruction within a few years—a policy little better, in fact, than cutting down a fruit-tree for its fruit; which appears more especially irrational when we consider how easily the advantage could be reaped without incurring the enormous waste, by a regular system of burning at times when, as after the first autumnal rains, and more especially in early spring, the ground is too wet to allow of injury to the roots, while yet the grass and weeds may be burnt off low enough to serve all practical purposes, and to destroy, at the same time, the Black Jack and Post Oak undergrowth, which is equally fatal to the range, with the fire itself. For the latter purpose, the burning in early spring, when the sap is rising, would be the most favorable time.[25]

In a study¶ entitled "The Recent Intrusion of Forests in the Ozarks," Beilmann and Brenner dealt particularly with the eastern and northern portions and concluded that "Within historic times this vast region was a prairie, or at least park-like in that the trees were widely spaced and confined to the water-courses and drainage-ways." In explanation of the change from prairie to forest in this transitional region they included among the factors "the extremely important rôle of fire in the perpetuation of the grassland at the expense of the trees."[26]

In 1939, H. C. Hanson summarized much of the modern research literature on the effects of fire, especially upon trees. In general, he indicated that in transition country the effect of fire was to discourage trees and favor grass, yet he warned that this was not necessarily the case, as fire increased the sprouting of some woody plants. White pine was badly damaged or destroyed by fire, but long-leafed pine was resistant, fire contributing to the savannah form of vegetational structure under some conditions.[27] Braun-Blanquet, the leader of the Montpellier school of plant sociology, in Europe, took the position that "Fire is particularly destructive upon very thin, sterile soils and especially in the transitional region between forest and prairie, where both types of vegetation are struggling for control."[28]

The role of aboriginal man in influencing the mesquite problem has not been given an all-out investigation. Insect infestation should not be ignored.[29]** Any pretense at drawing conclusions now would be premature, but the preliminary analysis stated here should suggest possible investigations. Such experimental work as has been done indicates that the problem is complex, and no easy solutions are to be anticipated. The point of this discussion is to emphasize the fact that the historical perspective on the whole question is seriously deficient, and that there is need of a comprehensive historical re-examination of the mesquite problem on a scale comparable with Roe's *North American Buffalo*.

¶ Some adverse criticism may be made of the Beilmann-Brenner study, although the major conclusions would not be changed. First, a more critical examination is in order of some documentary material used. For example, modern scholarship does not accept the view that the Coronado expedition of 1540–41 reached the Ozark country, and therefore the accounts of that expedition have no place in the evidence supporting the prairie interpretation of the Ozarks. Second, no discussion is included of whether earthquake disturbance may have been a contributing factor through local topographical and drainage changes, or ground-water levels. To be considered especially would be the disturbance of December 1811–March 1812, rated by geologists as of an intensity equal to the San Francisco earthquake.

The disappearance of salt licks or comparable accumulations of salt occurred elsewhere, so the assumption of a climatic change is not necessarily essential to the Ozark phenomena. Local changes in drainage and ground water incident to white occupation need more careful investigation, not only here, but as a general problem.

** Bartlett reported that "The tree seems to suffer from the attacks of insects in a similar manner with the locust." Harvard pointed out that insects laid eggs in the mesquite seeds, which destroyed germination.

X. Sagebrush and Cactus

The sagebrush problem is similarly the subject of contradictory treatment in the printed documentary material. Likewise, the tendency is to attribute sagebrush to overgrazing, in spite of the fact that many of the most significant descriptive accounts of the earliest explorers and travelers run to the contrary.[30] An interesting illustration is the following from a letter, written July 2, 1854, by the Rev. W. F. Boyakin, en route from St. Joseph, Missouri, westward, to a correspondent in St. Joseph:

> the whole country is from the South Pass to this place, one boundless sandy plain, for miles every way, stretching as far as the eye can see, with nothing but the wild sage to break the monotony; over which roll oceans of sand uplifted by the roughest winds, fairly darkening the horizon from ten to four o'clock every day, making traveling truly disagreeable.[31]

The cactus problem falls into the same category of treatment as sagebrush in most range conservation literature. But studies by C. W. Cook, and by G. T. Turner and D. F. Costello, demonstrated that the cactus infestations were not the result of overgrazing, but were related to insect-climate-plant relations.[32] Overgrazing must bear justly the responsibility of a number of evils, but it has become a convenient scapegoat for a multitude of situations where the proper answer should be "Nobody knows."

XI. Tame Grass

As Americans, derived from English and continental European stock, were primarily a forest people, when they met the grassland of the interior of the continent, they misunderstood it in many ways. Forest man's concept of grass was conditioned largely by his experience where desirable pasture and hay grasses were not generally native soil cover, and had to be cultivated like any other field crop. That outlook upon grass persisted tenaciously after the grass country was actually being occupied. At least three variant misconceptions,

separately or in combination, are important to historical perspective: First, that prairie and plains grasses were inefficient in their utilization of soil and available moisture; second, the conviction that the prairie and plains grass cover always changed fundamentally in composition under domestic pasture; third, that prairie and plains grasses would not survive domestic pasturing. Forest man proposed and eventually undertook to introduce and cultivate in the west the tame grasses he had learned to depend upon in the forest country—timothy, orchard grass, bluegrass, etc., and the clovers—and to search for still better grasses.

Thus Edwin James wrote as follows:

> There can be little doubt that more valuable and productive grasses than the native species can with little trouble be introduced. This may easily be effected by burning the prairies at a proper season of the year, and sowing the seeds of any of the more hardy cultivated gramina. Some of the perennial plants common in the prairies will undoubtedly be found difficult to exterminate, their strong roots penetrating to a great depth and enveloping the rudiments of new shoots placed beyond the reach of fire on the surface. The soil of the more fertile plains is penetrated with such numbers of these as to present more resistance to the plough than the oldest cultivated pastures.[33]

In his "Notes on Nebraska" printed in 1852, Thomas Jefferson Sutherland, a Nebraska Boomer, agitating the opening to settlement of the Indian country, the present states of Kansas and Nebraska, advocated the planting of bluegrass. "On all of the lands of the eastern part of the Territory bluegrass in luxuriant growth may be produced; and there are spots of land scattered all over the plains of the west, possessing the requisite fertility of soil, for the growth of bluegrass, in any desired perfection."[34]

In 1883 Shaler published a paper on the "Improvement of the Native Pasture-Lands of the Far West." He advocated a search of other areas of the earth having similar characteristics, but also he made the following statement: "With the poorest grasses there are generally wide interspaces between the tussocks of high growing species. If these intervals could be filled with other forage-plants, the consequences would be a greater amount of food per acre. . . ."[35]

In Kansas, E. M. Shelton, professor of agriculture, 1874–90, at the Kansas State Agricultural and Mechanical College, was in most respects an unusual man, but he could not rid himself of the notion that tame grasses must be introduced to replace the native wild grasses. At the same institution, in 1887, W. A. Kellerman challenged the assumption that the composition of the native grass cover was undergoing a change, the tall grasses driving out the short grasses. He doubted whether the vegetation was changing.[36] The form of his remarks indicates that he was thinking especially of floristic range of distribution rather than quantitative density of the several species within the vegetational structure under fluctuations of climate.

Again, since the drought period of the 1930s much of the same debate, with suitable variants, has been carried on in connection with regrassing programs in the Western range country. For the most part, the decade of the 1940s has been favorable weatherwise for such operations, but prolonged drought may compel some revaluations. Once more, it might be appropriate to urge that comprehensive historical studies of the whole problem of grass introduction into the grassland are yet to be done.

The quotation already given from Edwin James' tame-grass proposal provides an excellent springboard for discussion. So far as a bare suggestion of the introduction of tame grasses was concerned, the first sentence quoted would have been sufficient, but the remainder of the paragraph provides the highly significant context, the clues to the conceptual equipment with which James viewed the problem.

The agronomist in James suggested how the seeding operation might be accomplished with the least effort—burning. But as an experienced forest man he recognized the possible difficulty of killing certain deep-rooted plants by the use of fire. Also as a forest man, who was evidently acquainted with plowing among roots of trees and brush, he was impressed by the numbers and formidable character of woody root growths of the plains country. As a technologist, James envisioned the difficulties to be met in attempting to turn such plains sod with the iron-shod wooden plow or the cast-iron plow of the period. The numbers of the roots and the "resistance to the plough" gave him pause. Furthermore, from an agronomic point of view, James was thinking of clear-tilled fields of grass, in which a simple-stand crop was to be grown—free of "weeds," of course. But as an ecologist James was deficient, in spite of the remarkably accurate and comprehensive character of his observations, in recognizing and recording all these facts about the strange country he was visiting for the first time. The thought did not occur to him, apparently, that the presence of these plants in such numbers, and the character of their woody roots and their deep penetration constituted a veritable ecological system, and that the wide spacing of the grass plants on the surface,

dividing space with the many species of forbs, was an integral part of that system. Neither did the thought occur to him, apparently, that to destroy any part of it would work a fundamental change in the whole system, soil and all, as deep, at least, as the deepest penetration of any root. This was a century before Tansley formulated the concept of the ecosystem, and for all of James' remarkable insight into so many things essential to this grassland system, so strange to his forest mind, he did not anticipate anything of the larger concept.

XII. Conclusion

As the central purpose of this paper is to point out the role of history in a research program,†† the discussion may properly return to the question of method. Casual or random excursions into historical material to find data that appear to fit a preconceived frame of reference, or to serve a particular purpose, are not only not sufficient, but such procedure is more likely than otherwise to lead to erroneous conclusions. Although the contention may appear paradoxical, to serve their purpose historical studies must have no purpose. In an immediate functional sense they are useless, and must be useless in the same sense that Max Planck said of science: "Scientific discovery and scientific knowledge have been achieved only by those who have gone in pursuit of them without any practical purpose whatsoever in view."[37] This dictum applied to science is equally valid as applied to history.

The documentary evidence for historical studies of the kind proposed here is so contradictory and so fragmentary that the strictest precautions and safeguards must be exercised in its use. It is possible to find evidence, especially when removed from context, to make a show of proof for almost any predetermined conclusion. If "quickie research" should arrive at a sound conclusion, it would be purely accidental. Sound and comprehensive studies are more likely to require a commitment to many years of systematic collection and analysis of data in full context. Furthermore, data must be tied explicitly to time and place. Each spot is unique in an absolute sense. No one can predict what time a historical project may require for completion. Although, upon occasion, a few months may suffice, the minimum requirement is more likely to be measured in years, or a long lifetime. In a perfectionist sense a historical project can never be finished. But, within the realm of the possible, historical work, as intellectual enterprise, can be so comprehensive and complete as to render difficult, without danger of immediate exposure, any flagrant misuse of evidence by propagandists. History need not be written by historians, but whoever writes it must assume the obligation of doing it by the most rigorous historical methods.

Two other conclusions are pertinent. Every vegetational map must be dated historically. By that statement is meant that any description of vegetation is valid only for a particular historical time, and what exists at that particular time is the product of the whole situation, which must include man, whether he be primitive or so-called civilized. The other conclusion is closely related, because the vegetation of any specified time and space is an aspect of the ecosystem. If upon no other grounds, the recognition of man as a factor in the system precludes any possible recognition of the concept of climax for vegetation, for animals, for soil, for man, or for the ecosystem as a whole. Also, the traditional concept of succession must be revised. Change is the overriding fact, but disturbance of any rigid, orderly sequence is incessant, and, obviously individual events, whether acts of nature or of man, as they impinge upon any particular spot, are unpredictable. A temporary tendency toward establishment of a "steady state" is certain of interruption by the intervention of unpredictable events, and, for emphasis, man must be specified explicitly because of his characteristic of worrying about the future and of devising ways and means for trying to manipulate nature. What is said about vegetational mapping applies similarly to animals, to soil, and to man himself. The state of the ecosystem at any particular moment is the product of the factors of the past that have shaped it; and the state existing at that specified moment is the parent material upon which, or through which, succeeding states are formed—an indeterminate system.[38]

The present paper is focused upon the past, which is peculiarly in the jurisdiction of history. But there are no Rothamsteds in the United States —experimental areas, with carefully kept records of land use over more than a century. It is time such fundamental experimental areas were being set up in order that the same statement cannot be made in 2053.

The ideal of intellectual enterprise, whether history or science, is to attain at one and the same time, both depth and perspective. To attempt either without the other can lead only to futility.

†† Since this was written a Unesco report has been issued submitting some significant recommendations relative to research programs for arid and desert regions: *Arid Zone Programme*: Report of the fourth session of the Advisory Committee on Arid Zone Research. Royal Society, London, 29 September–October 1952. Unesco/NS/103 Paris, 31 October 1952.

In many respects, twentieth-century specialization has reached that barrier, and in some areas more seriously than in others. The development of ecology, or the ecological point of view, is a healthy recognition of that fact and an earnest of a determination to do something about it.

Although the fashion has been set by major segments of the intellectual world to dismiss Aristotle and the ancient philosophers as outmoded, there can be no harm done, and possibly some good can be done, in reminding moderns of the great Aristotelian concept expressed in the phrase *in potentia.* Whatever line of descent from Aristotle one may choose to follow to the present, a positive philosophical outlook is still defensible, grounded in that concept. The potentiality of man to solve problems has not yet been exhausted, and the potentiality of the resources latent in the earth to be brought into the horizon of usefulness is still beyond the power of man to conceive. The key to the situation is not the earth, but the minds of men determined to realize their own potential in act.

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The Interplay of Social and Internal Factors in the History of Modern Medicine*

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THE history of any science may be best interpreted in terms of two major aspects: first, that of the internal trends in the science as such; and, second, that of the relations between the discipline and the social environment in which it evolved. This distinction is not absolute but is useful for most purposes, provided that the two categories are broken down into meaningful components. The internal aspect includes the assumptions and objectives of scientists, their approaches (questions asked of nature), methods employed (logical and technical), foci of interest, necessary sequences in discovery, evolving ideas, and so on. Also internal to science is the role of individual genius. Pertaining to the environment, in contrast, are the social, technologic, and philosophic backgrounds. This also involves professional and institutional circumstances, and independent developments in other sciences.[1]

It was usual, during the nineteenth century, to write the history of science largely in terms of the former (internal) aspect. Although some attention was accorded to professional circumstances and to philosophic backgrounds, little heed was given to the intricacies of the total social milieu. This oversight or indifference was carried so far, at least in the case of the medical sciences, as to imply that these developed within a social vacuum. In order to correct this tendency, much attention has been devoted in late years to the "social relations" or the "social history" of science;[2] but this effort has in turn been carried to extremes. One would gather, from some recent works, that the development of the sciences was little more than a function of the general culture of any given time and place.[3]

It is not the present purpose to linger with either of these extreme interpretations. The view here taken is, rather, that the history of science can be understood only in terms of a constant interplay between internal logic and environment.[4] The omission or even the relative neglect of either of these—however helpful for immediate analysis—will distort any final picture. One may hold, no doubt, that the internal story of a science is of primary concern because this is the most distinctive aspect of its history. It is this which makes biology, biology, and not just an indistinguishable thread in a larger pattern. Yet even this view may involve emphases not fully justified by the evidence. There seem to have been times when environmental factors were indeed more potent than internal drives in pointing science in a particular direction.[5]

The analysis of modern medical history which follows is intended to present (to bring out, as it were) the interplay in that particular field between environmental and internal developments. The sequence employed is more a topical than a chronologic one, and the treatment will be as comprehensive as brevity permits.

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One may recall, to begin with, the influence exerted on medicine by the European cultural environment as a whole. During the early modern era, pervasive changes—to which science was continuously related as both cause and effect—stimulated and transformed various aspects of "natural philosophy." Medicine[6] then shared with other sciences (as these are now recognized) the benefits which resulted from certain trends in European life—from trade expansion, the rise of the middle class, the revival of Greek science, new intellectual outlooks, and so on. One need not review all aspects of the sociology of knowledge, or the varied implications of new perspectives, in order to recall the advantages which society gradually extended to science during this epoch. Certain it is that, by 1750, scientific activities were carried on within a social and intellectual setting which was far more conducive to discovery than had been that of the Middle Ages. And it need hardly be added that scientists, taking advantage of this, had by that time achieved remarkable results in both basic and applied investigations.

Yet the rate of advance varied widely in different fields. If one accepts the use of quantitative methods as a measure of progress, for example, it is clear that dynamics had emerged on a level by 1650 which chemistry did not attain until after 1750, and which clinical medicine had hardly reached even by 1850. This meant that between these dates different sciences were operating on distinct levels of method and of achievement, despite the fact that all were immersed at any given time in a common cultural environment. Such contrasts could be ascribed (1) to differences in the relationship between the common environment and one science as compared to another, and (2) to differences in the respective natures of the various sciences as such.

In the case of medicine, the first theme noted—the particular relation with the surrounding culture—was of unusual significance. It will be recalled that, despite conditions relatively favorable for science by 1750, there was still much to be desired in this connection. It is true that universities were maintained with the help of church, state, and private endowments, and that professors in certain of these were expected to pursue original work. But there was little or no direct financial aid for research itself. The very establishment of scientific academies was evidence of the inadequacy of the older institutions so far as science was concerned. In England, at least, few outstanding scientists taught in the universities, and the recruiting of future investigators was anything but systematic.

No doubt the rather casual manner in which research was then supported was more effective than it could be today. The prevailing arrangements were fairly well adapted to the needs of the physical sciences, since most of the latter were still in a relatively simple stage of development. Research procedures were not complex, the necessary technical facilities were simple and inexpensive, and little specialized training was required of investigators. Under these circumstances professors could manage reasonably well without "outside aid," and self-trained amateurs could and did do outstanding work.

The relation of medicine to the cultural environment, however, was less satisfactory. Unlike the physical or the general biologic sciences, medicine dealt directly with the most vital interests of mankind—with birth and with death—and out of this situation arose a whole series of peculiar difficulties. Most obvious, first, was the manner in which the use of the human body (as basic subject matter) was hedged about by all sorts of moral taboos. Physical scientists could do as they pleased with test tubes and pendulums, but physicians must not experiment with living men except within very narrow limits. Popular opposition to the dissection of even dead bodies lingered into the nineteenth century, and some abhorrence of autopsies and of animal experimentation persists to this day.

Physicians could, of course, learn something about disease by a passive observation of the sick and by cautious experiments in treatment. But sound generalizations must be based on many cases, and the traditional "solo" form of medical practice did not enable a physician to see more than a small number of patients. What was needed was an institution in which many cases could be studied; that is, the hospital. Hospitals, however, had been founded chiefly for humane rather than for scientific ends, and it was not until the nineteenth century that many of them were so organized as to be available for systematic investigations.

If medical men surmounted the obstacles noted, moreover, they faced still another difficulty inherent in what Roger Bacon had once called the "nobility" of their materials. This was the fact that physicians were under constant pressure to get results quickly. This was not usually the case with physical scientists, because the latter's findings were rarely of vital concern to the public; hence, physicists, even though seeking "useful knowledge," could suspend judgment and proceed with due caution. But death would not wait, and so men desired that physicians reach conclusions without benefit of real verification. The insistent need for curing illness had been

present throughout the centuries, when the state of medical knowledge was such—we can now see—that it could not possibly meet this demand. Yet the attempt had to be made and, what is more, men ever wished to believe that it had at last been successful. Such wishful thinking led some physicians along the way of groping empiricism, while it tempted others to go to the other extreme of unverified speculation or sheer dogmatism in medical thought.

In addition to the difficulties imposed on medicine by humane considerations, there were further obstacles inherent in professional traditions. In the case of physical science, there was no large and ancient guild whose organization or vested interests might retard an effective pursuit of new science. It was far otherwise in medicine. Consider, for example, the situation in the United States during the nineteenth century. In this country, by guild tradition, there were rarely any full-time professors in medical faculties before 1900. Within the universities, professors of physical science could give all their time to teaching and investigation; but medical instructors were selected from among the best known—and therefore the busiest—practitioners. Such men could pursue original work in their spare moments, as could any self-patron; but the truth was that they had few moments to spare when lives were at stake. Cynics may add that professional income was also at stake here, but this was not the whole story. Even a wealthy practitioner who need never worry about "that damned guinea" found it wise to seek a large practice for the sake of prestige.[7]

In other words, the traditions of the medical guild antedated modern research and only slowly adapted themselves to it; hence, few physicians devoted much time to medical science, giving themselves rather to the related art of medical practice. Comte summed up the situation, in 1830, in observing that the prospects for medical science were as dim as they would have been in astronomy if all research in the latter had been left to *its* practitioners—that is, to the sea captains![8]

One may conclude that, although medicine shared in advantages enjoyed by science at large, it also was handicapped by unsatisfactory relationships with its social environment. Since this lack of rapport was more or less peculiar to medicine, much of the lag in this field between 1600 and 1800 may be traced to it. But the slow pace of medical progress may also be ascribed in part to the other major factor; that is, to the internal nature or logic of medical science as such.

In the first place, biologic phenomena were in a sense more complex than were the physical. This was apparently not fully realized in the seventeenth century when, encouraged by the succcess of dynamics and influenced by the concept of the "animal machine," there was much enthusiasm for an experimental and quantitative study of the bodies of men and animals. But although the first results were encouraging, as in the discovery of the circulation of the blood, the iatrophysical and iatrochemical schools became bogged down by 1700 in a morass of obscure phenomena. It was easier in dynamics than it was in physiology to isolate problems that could be solved in terms of the knowledge and techniques then available. Hence the zeal of 1650 for quantitative concepts and procedures in medicine, however sound and prophetic in principle, was of small avail at the time.[9]

One may pause here to inquire whether this outcome really involved anything more than just another case of adjusting medicine to the surrounding culture—in this case, to the other sciences. For if the human body *was* simply an animal machine, then medicine merely called for the application of physical science to this body. And, in that case, medical science could be expected to advance only in the wake of the physical.

That there was some truth in this will hardly be denied. Biophysics presupposes an adequate physics, biochemistry an adequate chemistry. But this truism was not so applicable to early modern medicine as it is to that of the present time. This is because much of the significant research of the earlier period was done in pathologic anatomy and related clinical problems, and these fields could make little use of either chemistry or physics. Indeed, they usually involved only simple observation, without benefit of the experimental and quantitative methods already taken for granted in the physical disciplines. The significance of this is apt to be overlooked, if it is assumed that experimentation is the only method employed in modern science for observing nature.[10]

In other words, a great part of medical research prior to 1850 was still necessarily in the descriptive stage, even as was that in other biologic sciences. Now botanic and zoologic taxonomy presented real difficulties, but these were relatively simple in comparison with the complexities associated with the taxonomic stage of medical developments. It is when one considers this phase of the story that the problems inherent in medicine as such become more apparent.

Medical thought was in a confused state during the eighteenth century. There was some enthusiasm for Baconian induction and for the empiricism of

the Greek clinical tradition. But the Greek heritage also involved a theoretic, generalized pathology, and learned physicians felt it necessary to defend this "rational" theory against the skepticism of the "mere empirics"—a controversy which had likewise been inherited from classical times.[11]

Actually, much medical lore had had an empirical origin. Pharmacopoeias were the cumulative products of centuries of trial-and-error gropings after remedies. The one clearly specific drug known—cinchona—had had a folk origin. So had the first positive measure in preventive medicine—inoculation against smallpox. Yet the "rationalists" were correct in assuming that progress in medicine—as in any science—must in the long run be based on principles. And what could these be?

Since the purpose of medicine was usually assumed to be the prevention or cure of disease—rather than biologic knowledge for its own sake—it was assumed that medical thought must be based on a theory of the nature of illness. This theory was provided by the prevailing Greek tradition. Illness was usually viewed as a condition in which the body fluids or humors (blood, bile, etc.) were impure or out of balance. Once accepted, this theory led logically to a therapy of bleeding, purging, and other depletion procedures, intended to eliminate impurities or to restore balance in the "general state of the system." Names had long been given to the more obvious "clinical pictures" (smallpox, great pox, etc.), but there was little interest in disease identification. Since it was the state of the body which the physician treated, and as this seemed much the same in various types of illness regardless of any names employed, why bother about exact diagnosis?[12]

This view is not in itself to be lightly dismissed, as was the wont of medical critics during the past century. It involved some shrewd insights or at least inspired guesses; indeed, it may present us with one of the basic alternatives in outlook to which pathology will from time to time return. But the point here is that the ancient, humoral pathology, as still accepted in the 1700s, was both vague and unconfirmed. Professional discussions of its validity, or of that of opposing theories, were reminiscent of scholastic disputes rather than of an effort toward verification in the manner already established in the physical sciences. No knowledge was then available by which the humoral theory could be either proved or disproved. Clinical evidence was cited in its favor; that is, treatments based on the hypothesis were often followed by recoveries. But this was obviously the logic of *post hoc, ergo propter hoc*. Such reasoning would hardly have been accepted *if* medicine had developed in a social vacuum, but here again one must recall the dense atmosphere of human hopes and fears within which physicians actually carried on.

Equally vague and unconfirmable were prevailing ideas about the causes of assumed humoral conditions in the diseased body. Most physicians, as late as 1850, had only confused notions on this score. They spoke, as had the Greeks, of unhygienic habits, of heredity, of poisons in airs and waters, of contagion, and of what now would be termed psychosomatic influences. A few were even convinced that infections could be traced to minute "insects" or animalculae. But these explanations were rarely verified in any exact manner.

Indeed, as long as one general state of the body was assumed to underlie all illness, there was no great interest in causal factors (etiology). For, since illness was viewed as basically of the same nature in all cases, physicians focused their attention on this condition. What had originally caused the bilious, the dropsical, or the feverish state of the system was not so important as was the question of how one dealt with such a condition once it had appeared. After all, it was for this curative function that physicians were desired in society. Here one encounters again a limiting social circumstance. Physicians did write at times on preventive hygiene, but laymen usually felt that this was a matter of folklore or common sense. By tradition—and this is still all too true—physicians were called in only to cure acute illness. And at that stage, etiology seemed to the humoralists to be largely an academic matter.

There was, however, another Greek tradition which taught a doctrine essentially different from that of humoralism. The so-called school of Cnidos had held that there was no one pathologic state common to all illness. Rather, were there many distinct diseases; from which it followed that there were many distinct causal factors—some or all of them specific for particular diseases. Means of prevention, cures, and prognoses were also likely to be of a specific nature.[13] From this viewpoint, the first purpose of physicians was to discover these different diseases; for one could hardly seek the cause and cure of a particular condition until this entity was itself identified.

From the time of Galen until the sixteenth century the humoralist tradition dominated medicine, and the Cnidian view was recessive. Just why a few physicians then revived emphasis on the latter is not clear. Increasing knowledge of non-Galenic, Greek medical literature may have had something to do with it. There were always clinical

phenomena, of course, which suggested differences in types of illness; and it is conceivable that a renewed attention to these differences resulted from a slow but pervasive improvement of observation in general. More definitely, it has been suggested that the discovery of a remedy which was helpful against *only one* type of illness implied that this type ("clinical picture") must be distinct from all others. The only striking instance here was the discovery that cinchona bark was a specific for a certain type of fever (malaria), but this one instance made a deep impression on physicians during the seventeenth century.[14]

Whatever the explanation, the doctrine of specificity was clearly revived and emphasized during the latter part of that century. The most sweeping presentation was that of the English clinician Sydenham, who held that diseases were as real and diverse as were species of plants and animals. Each disease entity had its own cause, its own natural history, and even—if these only could be found—its own cures.[15] The optimism implicit in this outlook has not always been fully appreciated, but it must thereafter have had an increasingly stimulating impact upon medical thought. Instead of continued dependence on the shopworn and static doctrines of humoralism—which had all the answers—those who accepted the concept of specificity could envisage new and promising discoveries all along an advancing front in medicine.

This is not to claim that it was easy at first to identify diseases, to say nothing of finding specific cures for them. It was difficult even to determine what criteria of identification could be employed. Sydenham and his successors defined disease entities largely by symptoms—a procedure which had always been vaguely followed in giving names to different patterns of illness. But when attempts were made to do this in a more systematic and exact manner, the effort bogged down in the multiplicity of symptoms and their combinations. The nosology texts of the later 1700s listed and classified almost two thousand so-called diseases, but these lists involved little more than names for that number of symptom combinations. So confusing did this situation become that some medical leaders of 1800 maintained, or returned to, the view that there was only one pathologic state in all forms of sickness.[16]

Fortunately, the energy imparted by the concept of specificity finally carried medicine through the nosologic maze. The route followed was opened up by the study of human anatomy. This field, cultivated in classical Alexandria and revived during the later Middle Ages, was brought to a flourishing state during the sixteenth and seventeenth centuries. Representing basic research, anatomic studies had been motivated in part by artistic interests or simply by intellectual curiosity. But they eventually came to the rescue of a field long confused by its "practical" focus on the nature and treatment of illness.

As research in normal anatomy progressed, it led by an almost inevitable logic into a discovery of morbid (pathologic) anatomy as well. The view that there was a relationship between disease and structural changes within the body was first expressed in Alexandria, but was thereafter lost to sight during the long dominance of generalized humoralism. Perhaps the possibilities were never entirely forgotten in obvious instances; for example, it was suggested even in medieval autopsies that crude obstructions might occasion sickness. But normal anatomy had first to be revealed before its implications for morbid anatomy could be demonstrated. From the sixteenth century on, great anatomists increasingly called attention to pathologic phenomena encountered in the course of their dissections.[17]

One might now think that such observations would have immediately suggested a relationship with disease patterns. Actually, however, it required more than two centuries to put two and two together in this fashion. (It is a truism that scientific concepts which now appear simple enough were originally most difficult to formulate.) In this case the delay in relating morbid findings to disease may probably be ascribed to two circumstances, the one internal and the other external to general medicine. Within medical science, it was necessary to know something of the function as well as of the form of organs in order to relate structural changes to the abnormal functioning observed in illness![18] Such a knowledge of physiology also evolved logically from earlier studies in anatomy, but this was a gradual process.[19] Not until the later eighteenth century, at best, was physiology adequate for the purpose here noted. The first physician systematically to correlate morbid findings with disease patterns was the Italian Morgagni (1761); but even then this approach was ignored for decades by physicians in general.

One may say that, after Morgagni's time, the knowledge of morbid anatomy on the one hand and of physiology on the other was potentially adequate for revealing a relationship between structure and disease. But physicians, still thinking in terms of a merely humoral, generalized pathology, had to be prodded in some way in order to recognize the possibilities of this new orientation.

The immediate stimulus, in this case, seems to have come from the surgeons. As Temkin has pointed out, surgeons—in the nature of the case—had always dealt with structural conditions. It is true that they were usually concerned with such external situations as fractures or superficial excisions, since a pathology of humors did not call for interference within major body cavities. But all that was needed in order to provide a complete localized, structural pathology was to project surgical attitudes regarding obvious injuries to internal lesions as well.[20]

Now this transfer of surgical attitudes to physicians was delayed by social circumstances; namely, by the separation of these two guilds. Surgeons were long viewed as professionally inferior, and physicians were therefore not inclined to look to them for guidance. Fortunately, professional barriers began to be lowered during the late eighteenth century, and some leading physicians became in consequence familiar with surgical outlooks at that time. There is contemporary testimony by internists that it was this familiarity which finally led them to think in terms of a structural pathology. Such a reorientation was most apparent in the so-called Paris school after 1800, which was largely responsible for establishing the localized, structural pathology as the dominant doctrine over the ensuing century.[21]

It was realized, after this time, that a correlation of ante-mortem symptoms with post-mortem, structural findings could reveal disease patterns which were more clear-cut and distinctive than were those defined by symptoms alone. A great impetus was thereby given, not only to autopsy studies, but also to the improvement of clinical investigations. As has often been said, physicians prior to 1800 *observed* their patients; thereafter, they began to *examine* them. The introduction of improved research methods in clinical medicine subsequently owed much to the cultural environment, as when mathematics made statistics available and when technology produced achromatic microscopes. But such equipment would have been useless in medicine without the revolution in its outlook.[22] Actually, certain potentially useful instruments (thermometers, pulse watches) had long been available, but had not been employed by physicians because they had no interest in exact clinical observations. Not until they began to *look* for correlations between subtle, bedside phenomena and lesions did they desire such instruments. And when they did so desire them, the first ones employed—such as the stethoscope—owed nothing to current physics or technology.

Between 1800 and 1850, progress was made by the new procedures in the identification of many diseases as these are still recognized. In the place of the old humoral theories, or even of vague symptomatic notions like "inflammation of the chest," there appeared such relatively specific concepts as "pneumonia" and "bronchitis." And instead of confused symptomatic notions of various "fevers" (intermittent, continuous, remittent), there emerged relatively clear-cut concepts of typhus, typhoid, malaria, and the like.[23]

In consequence, medical research was ready, by about 1850, to undertake the next step, which Sydenham had long before envisaged; that is, to seek out specific causal factors and cures of these now-identified diseases. Here, again, research was aided by methods or instruments that could not have been employed prior to identification. Thus the microscope could not have been used in a search for specific pathogenic organisms until the diseases to which these were related were clearly recognized. Pasteur could never have found organisms which were causal factors, had he thought in terms of such vague conditions as "biliousness," or "inflammation of the chest."

Up to this point, medical opinion had varied on the almost metaphysical question of the ultimate nature of diseases.[24] Were these, as Sydenham held, objective entities—even as plant or animal species? Were they something which invaded men's bodies from the outside—a notion reminiscent of ancient demoniac lore? There was considerable resistance to this "ontologic" concept, even among pathologists of the later nineteenth century, who tended to think of disease as simply a form of bodily response to stimuli. Instead of viewing this as involving the bodily "system" as a whole, however, they now thought of response in particular parts—in organs, tissues, or cells, depending on the historic stage of research involved.

When specific, pathogenic microorganisms were discovered after 1875, however, bacteriologists at first viewed these as solely responsible for related infections. And as long as typhoid bacilli were thought of as *the* cause of typhoid fever, it was easy to think of this disease as an objective entity—incarnate in the bacilli, so to speak, and loose in the community. But subsequent developments in immunology and other fields reduced the role of pathogenic organisms to that of merely one factor among many, and the concept of disease as bodily response has once again become dominant. These trends pertained largely to thought within medicine, but they were also conditioned—delayed or accelerated—by the changing environment in

which medical scientists found themselves after 1800. Hence, one may turn again, at this point, to the interaction between internal drives and the social setting of the nineteenth century.

If space permitted, much could be said about the more subtle influences exerted by society on medicine during this later era. Prevailing philosophic outlooks—British empiricism, German idealism, and French positivism—all had some bearing on the course of medical research, despite the fact that this was the period when science consciously sought to cut itself loose from philosophic backgrounds.

More obvious than the influence of philosophy was that exerted by changing social and economic conditions. Certain of these changes were favorable. Medicine shared with other sciences the various advantages ushered in by the industrial revolution—increasing wealth, urban growth, improvements in printing, transportation, and technology in general. These were the developments which made possible the organization of science as we now know it; with reference, for example, to libraries, societies, and journals. Various social factors, meantime, converged to produce the modern universities, in which research and training for research were happily combined. Medicine profited from all this, as it did likewise from the improved facilities and equipment which technology placed at its disposal.

Two nineteenth-century social trends were of peculiar benefit to medicine—the growth of cities, and the cultivation of humanitarianism. These combined to produce and improve the large, general hospitals. Although not originally intended for the purpose, this type of institution turned out to be a *sine qua non* for the very clinical and pathologic research which was so essential at the time. We would have had the hospitals without modern medicine, but no modern medicine without the hospitals.

On the other hand, some aspects of social evolution were less favorable to medicine. Consider, for example, the social reactions of 1825–75 to the expanding hospital program of clinical and pathologic research. As already implied, it is difficult to see how basic progress in medicine could ever have been achieved except along this line. Yet the program was centered for more than fifty years on the identification of diseases, rather than on their prevention or cure. Research men were so preoccupied with this "pure" research, which had no immediate prospect of utility, that they apparently lost interest in therapy. Moreover, their more critical temper led them to discard the older remedies, at a time when there was as yet little with which to replace them. A spirit of "medical nihilism" pervaded the best centers.

Insofar as this nihilism became known to the public, it was not calculated to inspire confidence in medicine. There is indeed evidence that this period, which we can now see was of the greatest promise in medicine, was one in which the public had the least confidence in regular practice. One of the indications of this was the proliferation of rival medical sects, such as homeopathy, "the botanic system" (Thomsonianism), and so on. These sects—like later osteopathy and chiropractic—preserved the old theses of a generalized pathologic state or of some single scheme of treatment, long after these oversimple formulas had been repudiated in regular medicine. Then they promised the cures which candid regular physicians no longer believed possible, and so appealed to a public which was often in dire need of such assurances.[25]

More serious than popular doubts, moreover, was the danger that neither philanthropists nor governments were likely to support a field having little apparent utility. Modern science had early been hailed as a means to acquiring "useful" knowledge; and if this was indeed its major purpose, why assist it when it failed to serve that end?

The answer to this query was not a simple one. Actually, little direct private or governmental aid was extended to pure research in medicine or biology prior to 1860. But in relatively aristocratic countries not yet dominated by industrialism, science continued to benefit from the deference long accorded to learning as such. During the Enlightment of the eighteenth century, such deference had increased and had focused, so to speak, on the universities. This can be best observed in the prestige enjoyed by German or other Continental professors, and in the support extended to them by their respective governments. It even became a matter of pride with some men that their research had no relation to "mere utility."

In relatively democratic countries, where businessmen became increasingly influential, the middle classes continued to encourage the pursuit of useful knowledge. Conversely, they had little desire to support basic research. This attitude could be observed to some degree in England, but found its extreme expression in the United States; where a "practical" people saw little reason for supporting the "idle curiosity" of pathologists or other pure scientists. It is hardly an exaggeration to say that, for most Americans, the word science

connoted simply applied science or technology.[26]

It is suggestive that in those countries where science was highly regarded for its own sake, there were notable achievements during the nineteenth century. This can be well observed in medicine, where the pre-eminence of French and German pathologists was widely recognized. Even more striking was the manner in which the French and Germans dominated medical bacteriology, as this field emerged after 1875. At the other extreme, again, was the experience of the United States, where—despite individual exceptions—the record in basic medical research was a minor one. Had the matter been left to this country, it is unlikely that modern medicine as we understand it would ever have evolved. At best, the process would have taken a much longer time. In this connection, as has been pointed out, American culture was related to that of Europe in much the same manner as Roman civilization had once been to that of Greece.[27]

The fact that technology made rapid progress in the United States might conceivably have led to parallel advances in basic science here. Certainly there are circumstances in which one can observe technology stimulating science, as well as vice versa.[28] This was true even in medicine; for example, when a knowledge of brewing and fermentation played a role in Pasteur's investigations. But whatever was true in special cases, the American story certainly indicated that technology *may* be successfully cultivated on a large scale without much benefit to basic research. It even suggests that an extreme devotion to applied science may interfere with, or at least divert attention from, basic science. Americans were quite successful at times in advancing medical practice, as in the introduction of anesthesia and in other contributions to surgery. Yet these achievements did little, prior to 1900, to stimulate investigations in medicine as a whole.

American attitudes were not likely to change until medicine—or any other science—demonstrated its immediate utility. Fortunately, this was just what happened in medicine during the later nineteenth century. This is an oft-told tale: of how pathology led into bacteriology, and the latter into rational sanitation and immunology. So far as infectious diseases were concerned, prevention took on new possibilities and public hygiene was transformed. Even curative medicine came to life, with the aid of chemistry and pharmacology, in chemotherapy. Meantime, pathology and physiology opened up the fields of endocrinology and metabolism, so that prevention, controls, or cures became available for the deficiency as well as for the infectious diseases.

Last but not least, surgery was transformed; not by anesthesia or aseptic procedures, as is often assumed, but primarily by the advent of the new pathology. As long as the old humoralism prevailed, surgery could never be more than a skilled trade auxiliary to medicine—for, after all, one could not operate on the humors. But once disease was conceived in reference to localized structures like the appendix, then the offending parts could be removed by operations. And when this was realized, surgery shifted from the periphery to the center of medical practice. The discovery of anesthesia and of aseptic procedures ensued, in an effort to make the most of newly discovered opportunities.[29] Here, again, it was the revolution in medical *thought* which initiated progress: better instruments and procedures followed in the wake of this reorientation.

Hardly had all these promising developments appeared before American policies toward medical research and practice began to change. Complex as the entire story is, one need only recall here the manner in which—after about 1890—licensing restrictions were tightened, sectarianism declined, and people began to seek admission to hospitals rather than to stay out of them. Medical research, meantime, played a Cinderella role. Completely neglected by both philanthropy and government before that time, it became the chief beneficiary of the great foundations during ensuing decades. And it is obvious enough that government also moved into this picture in recent decades.[30] This latter development, in turn, raised new problems regarding the relation of government to medicine as well as to the other sciences.

In all this, it was not American attitudes that had changed: it was rather medicine that had been transformed. Americans still demanded the useful, but they were now convinced that even the "purest" medical research might become helpful at any time. And, in consequence, society came to the aid of medical studies in a manner that promises much for the future.

Implicit in public confidence was a realization that the interplay between medicine and society had been profoundly modified during recent generations. In all that has been recalled here about the relations between medicine and its environment, the influences noted so far have been those exerted by society upon medicine. Nothing has been said, up to this point, about the possibility

of a reverse influence—that of medicine upon society.

The reason is obvious. There was little tangible evidence prior to about 1875 that medicine had had much impact upon society. In certain respects, physicians had undoubtedly been of service to their communities. They had inspired confidence, encouraged personal hygiene, and guided empirical sanitation. They had learned how to prevent smallpox, to control malaria, and to ameliorate other conditions. Surgeons, meantime, had been most helpful in emergencies. But over against these services must be set the damage done by generations of well-intentioned but too heroic practitioners. No one will ever know how many good people lost their lives on the altars of bloodletting and purging. On the whole, medicine had probably done more good than ill prior to 1875, but little measurable influence upon society emerges from such a balancing of values.

It is clear, however, that the whole situation has changed over the past seventy-five years. Medicine has become one of the major factors in unprecedented declines in death rates and in associated increases in life expectancy.[31] These trends, of course, have had their effect upon the composition of the population, notably in the rising percentage of elderly people and in the resulting increases in certain chronic and degenerative diseases. And this, in turn, not only presents further problems in medicine, but also imposes new obligations upon society—from hospital construction to social security.

In a different connection, medicine has confronted society with still another basic difficulty—that of medical costs. For the very advances that made medical care more desirable also made it more expensive; and out of this situation have arisen the public pressures for health insurance.

It is apparent, then, that the relations between medicine and society now involve—for the first time in history—a fully developed, two-way process. Each influences the other in many and significant ways. The drives within medical science itself, meantime, continue to operate as one of the most dynamic factors in the whole situation. In some respects, medical thought at the present time seems to be returning to some of the very concepts—such as that of a generalized pathology common to various diseases—which were repudiated during the 1800s.[32] But that is another story. The interplay between medicine and society continues as in times past, but the components and the results of the process are changing rapidly.

Footnotes and References

1. The distinction between internal and external factors is applicable to the sociology as well as to the history of science. See Barber, B. *Science and the Social Order*, 28 ff. (1952).
2. For the European literature see, for example, Merton, R. The Sociology of Knowledge. *Isis*, **27** (Nov. 1937), 493 ff. For more recent writings, there are a number of bibliographies in English; e.g., Leikind, M. C. *The Social Impact of Science*. Washington, D. C.: GPO (1945); and those in *Impact*, a Unesco periodical. Most useful is Barber and Merton. Brief Bibliography for the Sociology of Science, *Proc. Am. Acad. Arts Sci.*, **80**, 140 ff. (May 1952).
3. It is, of course, justifiable to present the history of scientific developments primarily in relation to the surrounding culture, as in Merle Curti's able work *The Growth of American Thought* (1943). But there is always some danger of misinterpretation unless the limitations of this frame of reference are clearly stated; see e.g., the author's review of this study in *Am. Historical Rev.*, **49**, 732 ff. (July 1944). In emphasizing the pursuit of "useful" knowledge and the impact of technology on basic science, Marxian writings tend to carry this "cultural" interpretation to extremes.
4. This is also the view of those concerned with the sociology of science; see especially Barber, *op. cit.*, 25 ff.
5. An instance of this, in the case of thermodynamics, is provided in Lilley, S. Social Aspects of the History of Science. *Arch. intern. hist. sci.*, (6), 376 ff. (Jan. 1949).
6. "Medicine" is used throughout in a broad sense, to include medical practice and institutions, the public health, etc., as well as the medical sciences.
7. Shryock, R. H. *Development of Modern Medicine*, 38 ff. (1947).
8. Comte, A. *Cours de Philosophie Positive*, III (1908); 148 ff. (1830).
9. Shryock, R. H. *Op. cit.*, 17 ff.
10. Barber, for example, although fully aware of the importance of simple observation in biologic science, declares that "The dynamic of modern science inheres in the proper interweaving of conceptualization and experiment" (*Science and the Social Order*, 19). Instances will be cited of the "dynamic" of interweaving conceptualization with simple observation as well.
11. Galen's comments are given in Brock, A. J. *Greek Medicine*, 130 ff. (1929).
12. Knud Faber has noted the expression of this view which is found in the concluding statements of the Hippocratic text on *Prognostics;* see his Thomas Sydenham, der englische Hippocrates u. die Krankheitsbegriffe der Renaissance. *Münch. med Wochschr.*, (1), 29 (1932).
13. Garrison, F. H. *History of Medicine*, 99 (1929).
14. Faber, K. *Op. cit.*, 29.
15. Rush, B., Ed. *Works of Thomas Sydenham . . . .* xxiv ff. (1809).
16. For example, Benjamin Rush, of Philadelphia, announced about 1800, as a new theory, that there was only one pathologic state. He was then hailed by many as having brought order out of the chaos of the nosologies. A long poem to this effect is preserved in the Rush *MSS.* in the Library Company of that city.
17. Long, E. R. *History of Pathology*, 77 ff. (1928).
18. Sigerist, H. F. *Man and Medicine*, 120 (1932).
19. Castiglioni, A. *The Renaissance of Medicine in Italy*, 49 ff. (1934).
20. Temkin, O. The Role of Surgery in the Rise of Modern Medical Thought. *Bull. Hist. Med.*, **25**, 248 ff. (May–June 1951).
21. Other social influences, of course, were brought to bear on the Paris school; such as those exerted by

empirical philosophers and by mathematicians. There is a large literature on this story. See, for example, Fosseyeaux, M. *Paris Médicale en 1830,* 97 ff. (1930); and Rosen, G. The Philosophy of Ideology and the Emergence of Modern Medicine in France. *Bull. Hist. Med.,* **20**, 328 ff. (July 1946).

22. Or, in more formal language, without a shift in the "conceptual scheme" (Barber, *op. cit.,* 19). But note that it was simple observation rather than experimentation which was here being directed by a new "conceptualization."
23. The evolution of these concepts can be traced by comparing old "bills of mortality" with the later lists of "causes of death."
24. Pagel, W. The Speculative Basis of Modern Pathology. . . . *Bull. Hist. Medicine,* **18**, 1 ff. (June 1945).
25. Shryock, R. H. Quackery and Sectarianism in American Medicine. *The Scalpel* (May 1949).
26. ———. American Indifference to Basic Science During the Nineteenth Century. *Arch. intern. hist. sci.,* (5), 50 ff. (Oct. 1948).
27. DeTocqueville, in analyzing this situation in 1835, thought that Americans would have developed basic science *if* they had lacked European aid. But he cited Chinese civilization as having failed to do this under those circumstances. *Democracy in America,* II, 518 (1904).
28. Lilley. *op. cit.,* 376 ff.
29. Sigerist, H. E. Surgery at the Time of the Introduction of Antisepsis. *J. Missouri State Med. Assoc.,* 169 ff. (May 1935).
30. Shryock, R. H. *American Medical Research: Past and Present.* 88 ff. (1947).
31. Winslow, C.-E. A. Communicable Diseases, Control of *Encyclopedia of the Social Sciences,* Vol. IV, 77 (1931).
32. Grey, G. W. Cortisone and ACTH. *Sci. American,* **182**, 35 (Mar. 1950).

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Mathematics and the Educational Octopus*

STEWART SCOTT CAIRNS

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THE perennial debate over educational policies and principles has in the past few years gone into an acute phase. Speakers and writers have attacked one another's views, and occasionally one another, with varying degrees of vigor. The dispute has sometimes waxed acrimonious and has frequently drifted into irrelevancies. In view of the acknowledged basic importance of the subject and in view of its intimate connection with future scientific developments, the writer believes that the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE should devote substantial attention to it. Accordingly, he proposed last April that at the December 1952 meetings there be an open and well-organized discussion of current educational practices and their relation to the sciences. Correspondence with a number of interested persons revealed, however, (1) that the atmosphere was unfavorable† to a reasonably objective debating of the issues, and (2) that there was insufficient time during the few intervening months to achieve the necessary conditions for a symposium appropriate to so important and complex a question.

In the hope that a suitable session for December 1953 may prove possible, the writer recommends that Section Q (Education) of the AAAS join with a few other sections in sponsoring a conference devoted to the subject of scientific training in the elementary and secondary schools. He further recommends:

1. That such conference be concerned with questions of content, teaching methods, administration, and policy control in the public schools, especially at the secondary level.
2. That it be planned by a special committee, representing various AAAS sections, on present tendencies in secondary education and teacher training.
3. That the planning committee endeavor to formulate some general recommendations and resolutions to be offered to the AAAS as a whole for consideration.
4. That such recommendations, resolutions, and other appropriate material be published in advance of the meeting or, if that prove not feasible, be distributed at the time thereof.

The present article is restricted primarily to mathematical training and is thus largely confined to one special phase of the battle against anti-intellectualism in the schools. The plight of the humanities is in a way even more serious than that of the sciences, for the latter currently have the mixed blessing of an artificial stimulus from defense efforts in favor of adequate scientific training of students. It is largely in the fields of literature, art, music, and the other humanities, however, that one would expect those with intellectual tastes, whether scientifically minded or not, to find occupation for the leisure time afforded by technological improvements and by those medical advances that continue to increase the average age of the population. Accordingly, let the scientists not neglect their humanist colleagues in the common struggle for the improvement of the public school program.

* This article is based on an address delivered December 30, 1952, before the Mathematical Association of America at Washington University, St. Louis, during the AAAS Annual Meeting.

† At the time of his address in St. Louis, the writer stated his belief that his proposal had been referred to Section Q of the AAAS and had there met with an unfavorable reception. This was a regrettable error, based on a misinterpretation of certain items of correspondence. The proposal had not even come to the attention of the officers of Section Q.

The Aims of Education and the Needs of Society

Prudence or modesty would lead most of us at first sight to shun these large and controversial questions and perhaps instead turn to our own scientific research problems, where we at least have the satisfaction of knowing with certainty when a solution has been reached; but the interests of mankind, the welfare of our children, and the prospects for further scientific advance should lead us to overcome our reluctance, especially when we consider that in dodging these questions we are, in effect, answering them. Decisions will of necessity be made somehow, by someone, concerning the educational program of our schools. We have only ourselves to blame if, by default, we permit vital policies to be determined on the basis of improper criteria in the hands of persons who, however well-meaning, have a biased and only partial comprehension of the issues at stake.

It is too much to expect that clear-cut, well-defined educational aims will be widely adopted for the guidance of our public school administrators. The guiding principles now most prominently advocated constitute a jumble of policies, subsumed under the general title of "life adjustment" education. Backed by powerful forces, in a manner to be described, "life adjustment" is gaining greater and greater prevalence in our country, so that it behooves us all to examine it carefully and decide on the extent to which it should be permitted to pervade the educational system.

Authoritative information on this movement is to be found in pamphlets issued by the Office of Education of the Federal Security Agency. One such pamphlet[1] says, "Life adjustment education is designed to equip all American youth to live democratically with satisfaction to themselves and profit to society as home members, workers and citizens." Although, in the form here stated, life adjustment is a great and noble aim for the public schools, grave trouble inevitably arises in its interpretation and implementation. Clearly, for the scholar and for the scientist, living with satisfaction to oneself and with profit to society will carry very different implications from those conveyed to a man devoid of intellectual interests. Along with other critics of modern educational practices, the writer recognizes that the narrower, better-defined academic aims of two generations ago are inadequate for our existing society, and that vocational training, social adjustment, and training for citizenship are necessary to modern mass education. These aspects of schooling should, however, merely supplement, never replace, grammar, literature, history, mathematics, and other essential elements of our intellectual heritage. The latter are subjects which, if neglected in the public schools, will be acquired by only a favored few, and the lives of the great bulk of our youth will be "adjusted" to a very low cultural level indeed. Is *this* our concept of democratic education? Rather, let the public schools assume the leadership in an intellectual revival. In a release dated September 20, 1950, the American Institute of Public Opinion reported that only 21 per cent of United States adults read books. Across our northern border, in Canada, the figure is 40 per cent. It is 43 per cent for Norway, and 55 per cent for England. In the light of these data, do we really wish merely to "adjust" our children to their environment, or shall we, using "education" in its true sense, lead them into something better?

The next large issue to be touched upon is that of the educational needs of society and of individuals. How vast is the need of the nation and the world for the truly educated? How widespread and how deep are the thwarted, perhaps unconscious, intellectual thirsts that now go unsatisfied for the sake of wholesale educational experimentation? One might suppose that mathematics, generally looked upon askance by the layman, would be a rare object indeed for such intellectual desires. Yet, counteracting this impression, consider the success of such books as Hogben's *Mathematics for the Million*. Kasner, with his *Mathematics and the Imagination*, a recent selection of the Book-Find Club, has fascinated many a layman, and other such examples could readily be cited. Let us hasten to agree with one objection to the foregoing argument, namely, that the entertaining style of the successful popularizer bears little resemblance to traditional classroom presentations. Many a future mathematician has been bored to extinction, and many a potential amateur repelled by the dullness of arithmetical and algebraic drill in the schools. Mathematicians have, however, played a leading role in efforts over the past fifty years to encourage the teaching of mathematics so as to appeal to the imagination and awaken the interest of students. Further success in these efforts, in which professional educators have cooperated, will depend on training public school teachers so that they will acquire a genuine appreciation for mathematics and so that their understanding will be developed well beyond the level at which they are expected to teach. Under present conditions many of them dislike and fear the subject and unconsciously transmit their feelings to their students.

Let us now pass on to one particular, measurable

phase of our national needs—the critical shortage of scientifically trained manpower. Throughout this article, the presentation of numerical data is minimized, because the retailing of figures is somewhat boring, because an unbiased canvassing of the available statistics would be a mammoth undertaking, and because the writer does not wish to assume the vulnerable position of those who select, from the wealth of statistical data, an array of persuasive facts in support of their personal views. Attention is called, however, to *Higher Education* 9, [7], [Dec. 1952]), issued by the Federal Security Agency, Office of Education. In an article on "Engineering Education in the United States," Henry H. Armsby concludes, on the basis of figures from the Bureau of Labor Statistics, that "there seems no immediate prospect of meeting the current acute shortage of engineers." The practical exigencies of this situation and the closely related shortage of scientists in all fields may prove to be the entering—or the re-entering—wedge for restoring some of the essentials of mathematics to their proper place in the schools. For, as Mr. Armsby points out, "engineering is based on mathematics and the physical sciences, and an interest in and aptitude for these fields is an important indicator of success in the profession. However, many studies have indicated that the pattern of high school subjects bears little, if any, relationship to success in an engineering college."

Even in peaceful times, the demands of modern society for engineers and scientists would be very heavy. As matters now stand, we need hardly be reminded that our national security hangs in the balance, and that, in the desperate struggle to preserve it, mathematics and science are crucial weapons. Our educational shortcomings are thus revealed not only in the light of a national disgrace but also as a dangerous source of weakness on the international front.

Attention is called to a statement, "Policy on Training and Utilization of Scientific and Engineering Manpower," by John R. Steelman, acting director of the Office of Defense Mobilization.[2] After a clear discussion of the problem, a set of recommendations is made (*a*) to employers of scientists and engineers, (*b*) to professional engineers, and (*c*) to educational institutions. The engineers are urged, among other things,

> To cooperate with educational institutions in studying the adequacy of existing curricula in the sciences and in engineering, in developing better teaching methods and in achieving maximum utilization of teaching facilities in scientific and engineering fields.

The educational institutions are urged

> To make special provision for students who possess the necessary aptitude for engineering work but who lack the courses in science and mathematics prerequisite for admission to the engineering college either because of the inadequacy of high school offerings or because their interest in an engineering career was developed too late in the high school program.
>
> To strengthen high school curricula in order that more high school graduates will be eligible for entrance into engineering colleges and to establish closer working arrangements between colleges and universities and high schools to the end that both high school students and the faculty will became more aware of the opportunities in the engineering field.

Responsibilities are then assigned to a number of agencies, including the Federal Security Agency, which is directed to

> Encourage and assist engineering colleges to make special provision for students who possess the necessary aptitude for engineering work but who lack the courses in science and mathematics prerequisite to admission either because of inadequacy of high school offerings or because their interest in an engineering career was developed too late in the high school program.
>
> Assist secondary schools in developing more adequate curricula and better teaching methods in order to provide students possessing the requisite aptitudes and interests with the fundamental education necessary for college work in science and engineering.

Growth of the Octopus

There is danger that a description of the octopus will seem to imply a wholesale attack on all colleges of education, on their entire faculties, and on all who participate in running the public schools. This is far from the intention. To avoid so false an impression, let us first cite J. G. Fowlkes, of the School of Education, University of Wisconsin. At a farewell dinner for Willard Spalding, dean of the College of Education of the University of Illinois, Dean Fowlkes is quoted[3] as having upheld arts and sciences training as the basis of teacher education, saying "It is nothing short of tragic that so many professors of education took their undergraduate work with a major in education . . . [took] a master's in education, and a doctorate in education. . . . My concern is great for the lack of sound general education of the elementary teachers in this country." As the guests departed, one educator is said to have remarked to another that they might as well have had Bestor[4] or Fuller[5] as speaker.

Another important gratifying event has been the valuable cooperation of the College of Education at Illinois with mathematicians and engineers in certain forward-looking projects in secondary school mathematics. These will be mentioned later in somewhat greater detail.

Least of all would the writer wish to appear to

be casting aspersions on public school teachers. They are our natural allies in the struggle for higher standards, and many of them have, not too vociferously, expressed a desire that they be permitted to teach something more substantial and to apply higher standards of performance than at present.

But we are neglecting the octopus. This creature grew as a byproduct of our national adherence to the principle that public education should be equally available to everyone. During the past few decades, we have seen widespread and well-motivated efforts to adapt curricula to the varied needs of all students. At the same time, school enrollments have experienced a tremendous growth, far out of proportion to the general increase in population. Since 1870, our secondary population has grown from 80,000 to 6,000,000—that is, by a factor of 75; and our colleges from 60,000 to 2,300,000, a factor of 38. Meanwhile the population of the country has merely tripled. This tremendous relative and absolute school expansion has created an almost overwhelming educational problem. Little wonder that the efforts to deal with it have fallen far short of perfection!

Economic and social developments, rather than a thirst for learning, have largely accounted for the phenomenon of a rapidly increasing fraction of the population wishing to continue school attendance to a more and more advanced age. The predominantly academic interest which dominated earlier secondary schools has offered little appeal to most of the inflowing students, and, accordingly, new curricula have been developed in an effort to satisfy the requirements of those with all degrees of ability and all types of interest. In such a period of change, it is natural that one extreme group of educators should advocate a close adherence to traditional academic studies, and that an opposing faction should desire to scrap the entire curriculum of the past and start from scratch. Neither extreme has quite won a complete victory, but we have been led much too far toward the goal of the latter.

Colleges of education have played a leading part in the recent development of our public schools. Given the enormous task of coping with unprecedented hordes of students, it was natural that such colleges should enjoy a vast expansion in both size and influence. Their products have flooded the educational system as teachers, principals, superintendents, and occupants of powerful posts in state offices of education. The certification requirements and conditions for advancement that have been established tend to strengthen the control of schools of education over the training of teachers, obliging the latter to include in their programs an amount of course work in educational methodology and allied subjects that seems excessive in quantity and deficient in substance. After these teachers embark, with a minimum background in subject matter, on their careers, they are encouraged, for the sake of professional progress, to devote some of their vacation time to additional study, with a continued slighting of subject matter in favor of educational workshops and more courses in methodology, psychology, and so on. There is little to marvel at here, for our elementary and secondary school system has so developed that true scholarly attainments are not particularly conducive to advancement. The premium is on the comparatively nonintellectual phases of school life, which, although important, make relatively light mental demands on either teacher or student.

The educationists have achieved power not merely through local and statewide influence, but also through nationally centralized activities. The U. S. Office of Education, in the Federal Security Agency, has proved an influential mouthpiece for the advocates of the life adjustment program. One pamphlet issued by that office[6] refers in the following terms to a report, *Cardinal Principles of Secondary Education,* by the national Commission on the Reorganization of Secondary Education.

> General, vague, and highly theoretical objectives such as formal discipline were rejected for such down-to-earth criteria as social utility, student interest, and provision for individual differences.
>
> Besides emphasizing a social and practical basis for the high-school curriculum, *this report seemed to receive wider acceptance because it was published and distributed by the U. S. Office of Education.* [Italics supplied.] It was the first national report to reach the rank and file of the classroom teachers. Moreover, the seven basic objectives of health, command of fundamental processes, worthy home membership, vocational education, civic competence, worthy use of leisure, and the development of ethical character, while general in nature, were sufficiently real to be well understood by the teacher whose task it was to work toward these objectives in the classroom. They were soundly based on such practical considerations as the needs of society, the nature and capacities of youth, and the findings of educational research.

For a more emphatic illustration of organizing power, we quote from *Life Adjustment Education in the American Culture,*‡ which reports a Work Conference on Life Adjustment Education, held in Washington, D. C., in October 1951:

‡ Federal Security Agency, Office of Education. This pamphlet is referred to hereafter as "L. A." The quotation is from p. 66.

It was agreed that: The State has a leadership function, not a policing function; the State colleges can serve as centers of stimulation; the success of a State-wide program of life adjustment education depends upon a well rounded task force at the State level which includes representation from the high school principals, the State Department of Education, teacher education institutions, and interested laymen.

It was also agreed that the States should exchange information regarding their action programs. The secretary of the national Commission on Life Adjustment Education was requested to prepare a list of no more than two representatives from each State in which systematic efforts are being carried on to improve secondary educational programs.

The Life Adjustment Movement

Let us turn next to a brief examination of this life adjustment movement, behind which such power is concentrated. The program goes also under the names of "core curriculum," "general studies," and so on. The term core curriculum, which is only vaguely defined, refers to a conglomeration of endeavors cutting across subject-matter lines and related to "real life problems." It is partly described as follows in *"Core Curriculum Development. Problems and Practices.*§

Some schools went still further in eliminating subject-matter lines. They believed that the school should do something about the problems which are persistent in the lives of adolescents as members of a democratic society. These problems are common to all youth and draw upon many different subjects for their solution. Working on them, it was thought, would develop the personal and social competence of youth. Also, the democratic processes of pupil-teacher planning and cooperative group work which would be used, should develop the habit of reflective thinking and skill in solving problems. Classes were organized on a block-of-time basis with one teacher in charge throughout the two or more periods (p. 5).

Perhaps the concept of this curriculum can best be clarified by quoting one of the several examples of activities to be found in C. C. (p. 20).

Construct and interpret a personal health record form. On the form, place such items as: height, weight, illnesses, accidents, habits of good hygiene (bathing, brushing teeth, washing hair, etc.), problems of health and appearance and plans for solving the problems. Use the form to keep a health record.

Subject fields: Agriculture, Arts, Business Education, Distributive Education, Health and Physical Education, Home Economics, Language Arts, Mathematics, Music, Science, Social Studies.

The other examples are of similar character. If you are feeling particularly strong, you might care to read them, and, as you do so, ponder on the grocery-store concept of mathematics that is being propagated. The authority most quoted in this pamphlet, judging from the index, is Harold Alberty, of Ohio State University. In a speech in Illinois last spring, he included the following topics under core curriculum: orientation to schools; home and family living; community life; contemporary cultures; contemporary America among nations; political, social, and economic ideologies; personal value systems; world religions; communications; resource development; human relations; physical and mental health; group and individual planning; science and technology; vocational organization; hobbies; education; war and peace; public opinion. What should be taught under these ambitious headings is nowhere spelled out. Coupled with this impressive mass of topics is the principle of pupil-planning, in accordance with which the class, by discussion and vote, decides, or helps decide, what to "study" next.

Although a strong aroma of the ridiculous pervades the above programs, let us not scoff at their objectives. Efforts to promote the moral and social development of students are of primary importance. Many behavior problems among capable students could, however, be cured by providing them with studies to challenge their abilities in an atmosphere where there is an immediate incentive to scholastic success. The effort to retain everyone in school beyond the age of legal compulsion may even lead to increases in juvenile delinquency, instead of to suitable life adjustment, as a result of the unrestrained and infectious activities of students artificially induced to remain in the public schools. From the viewpoint of the present article, there is more cause for concern in the power of core curriculum to displace essential studies, as illustrated in the following quotation from C. C. (p. 5):

Such classes, of course, had to replace subjects that were already in the curriculum, subjects that were required of all. English and social studies were the subjects usually chosen, with science, mathematics, art, music, or health added in some instances. These were basic or core subjects. The new program then became known as the "core curriculum" or sometimes as "general education." It had a distinct type of organization in the total school program; its content and methods differed widely from traditional courses.

Note that core curriculum replaces the very heart of the traditional academic studies. To quote from a later passage (p. 19), "Occasionally English or social studies is combined with mathematics or science; infrequently science and mathematics are the basis of core. . . . Core knows no subject matter lines and the problem to be solved may draw from any areas of the curriculum that can contribute to its solution."

§ Federal Security Agency, Office of Education (1952). This pamphlet will be referred to as "C. C."

To emphasize further the expulsive force of core curriculum, we remark that Professor Alberty has advocated beginning "core" in the junior high school with two thirds of the school day devoted to it, and continuing it through high school *and the first two years of college,* gradually reducing the allotted time to half a day. Meanwhile, incidental to such activities as the "health record" project quoted above, the students are expected casually to pick up the displaced studies. Even where core curriculum has not predominated, the diversification of the high school program has been accompanied by a growing freedom for the students to choose what they will study and what they will omit. Naturally, as the more exacting courses have become optional, they have lost favor and patronage. Advisers of students tend to warn them against the more difficult studies, lest they run the danger of frustration and failure. Warning lights hang particularly over mathematics and Latin, when the latter remains on the program at all. Because of the bad psychological effects of poor grades on *some* students, the standards for *all* students have been lowered. This is done partly in the name of enhancing the "holding power" of the school, which means making it so attractive in one way or another that all students will attend even after legal compulsion ceases. In general, drill is tedious, so it is dropped. Minimum demands are made on the memory. Why we should shun the development of memory as a tool is a mystery difficult to fathom! Students are supposed to learn to reason without knowing facts, but why is it that they should not be obliged to master such useful tidbits as the multiplication tables? In English classes, the parts of speech are either not taught at all or are supposed to be picked up in some incidental manner—perhaps after ignorance of them has hampered students in foreign language courses. Mathematical instruction has been generally depressed to the level of the mediocre, and essential topics have been progressively omitted. Powerful figures in the educational field repeatedly emphasize the acquisition by students of necessary but trivial arithmetical competencies to the exclusion of those fundamentals of algebra, geometry, and trigonometry required for the most rudimentary efforts in the field of science. The life adjusters use the lopping-off technique of Procrustes. The weakening of mathematics, along with the general suppression of other classical academic subjects, has been partly justified by the false argument that the studies in question are good only for the college-bound minority. Hence, by a gross misinterpretation of democracy, these subjects are crowded out, to the detriment of everyone. Indeed, the damage to those who do not go to college is particularly severe, since the others will at least get later opportunities to fill the more serious gaps, but the non-college-bound majority are denied even a proper high school education.

College entrance requirements are among the obstacles that have hampered the experimentation of the life adjusters. Some colleges have high admission standards; others, especially state institutions, have almost nominal requirements. The "adjusted" students can go to the latter. If a student in a high school dominated by advocates of "core" wishes to prepare for college entrance examinations, he swims against the stream. On all sides are schoolmates who, in order to become college students, are required to do little but live long enough, and in front of the class is a teacher who has not much time or incentive to help him out. It is rare indeed to find a teenager, however intelligent, who will work assiduously toward a distant goal while his companions are contentedly loafing their way through high school and into college, with no penalty involved save perhaps a mediocre progress report in which they are at once complimented and slapped on the wrist by being told they are working below their abilities.

Even the weakest of college entrance requirements are targets for the life adjusters. We quote from a pamphlet[7] issued through the Office of the State Superintendent of Public Instruction in Illinois.

> If a considerable block of courses must be retained in the high school to provide for the preparation of students who hope to go to college, the opportunity to re-examine the total high school curriculum and to replan the program in terms of the needs of all high school youth is thereby curtailed.

We also quote again from L. A.:

> It was agreed that college entrance requirements are often an obstacle to curriculum change, especially in small schools. Described were the Michigan agreement (which eliminates any required pattern of high school subjects for college entrance) and the proposals made in Illinois for determining college entrance (on the basis of a few basic tests and the recommendations of high school teachers). Some States reported that colleges accepted all high school graduates, but doubt was expressed that all graduates were actually equipped for college entrance (p. 69).

This last sentence belongs in the department of understatement.

It is interesting also to glance briefly at the so-called Eight-Year Study, used by many educationists as a justification for experimentation. Although it purported to be a controlled statistical study, Vol. I of the report, *The Story of the Eight-*

Year Study,[8] states: "Everyone invited to serve on the Commission was known to be concerned with the revision of the work of the secondary school and eager to find some way to remove the obstacle of rigid college prescription." Little wonder that this report as a whole is a gold mine for those seeking samples of statistical fallacies!

The encroachments of life adjustment on academic work do not stop with the high schools, or even with the college admission standards. The tentacles of the ever-growing octopus now stretch dangerously toward the colleges and the graduate schools, with state-supported institutions as the most vulnerable. This tendency is revealed not only in Alberty's desire for 50 per cent of the first two college years but also in the following passage (L. A., pp. 63–64):

> In order to prepare those who teach teachers to teach, we recommend to graduate schools
>
> 1. That a candidate for a doctor's degree be counseled as to his vocational adjustment, the kind of job he expects to get, and the kind of people with whom he will work.
> 2. That a college and university teaching minor for a doctoral degree include such subjects as mental hygiene, guidance, and supervised teaching.
> 3. That faculty members of graduate schools be equipped to handle general education courses.
> 4. That specialists in one field in the graduate school work with and understand the work of specialists in other fields.
> 5. That the graduate school provide real life situations for prospective college teachers under supervision.
> 6. That graduate students be encouraged to help plan courses and to evaluate their own work and the work of the instructor.

In this connection, let it be noted that administrative posts in institutions of higher learning are frequently filled of late by those with education degrees.

The Struggle with the Octopus

Numerous and vigorous, but sporadic and largely uncoordinated, have been the protests against the substandard educational fare offered to our children. In many communities, parents have made more or less futile efforts to bring about a strengthening of the curricula, so that their children would learn at least some of the fundamentals of grammar, mathematics, and other subjects once considered an obvious part of the program. Scientists and scholars have raised their voices eloquently and convincingly but, thus far, to little avail. These are, we trust, only the preliminary skirmishes before the battle proper.

It would be a task carrying us far beyond the bounds of the present effort to undertake a balanced discussion or even a listing of the principal events in the educational controversy. The writer's participation in the battle dates back less than a year, when a sequence of incidents, outlined below, on the University of Illinois campus led him actively into the fray.

Although no attempt is here made to give a general treatment of the modern educational controversy, the highlights at the University of Illinois, which is by way of becoming a spearhead for the attack, will be mentioned. The opening guns were fired by Harry Fuller, professor of botany, in a Phi Beta Kappa address to the Illinois chapter on May 10, 1950. The published version, "The Emperor's New Clothes, or *Prius Dementat*,"[5] appeared in The Scientific Monthly for January 1951. Some months later, Dean Spalding, of the College of Education, responded in a vituperative address‖ on the Illinois campus, during which Fuller was characterized as "The Bewildered Botanist." The next address in the spasmodic Illinois series was "Aimlessness in Education, or *Ex Nihilo, Nihil Fit*,"[4] by Arthur E. Bestor, of the History Department. The discussion at the end of Bestor's speech turned partly on arithmetical training, apropos of the desire on the part of certain educational extremists to remove the three R's as an essential part of every normal student's program. The writer was particularly irked by the apparent argument that, because a certain percentage of school children are incapable of mastering the multiplication tables, the more capable students, as well as the incapable fraction, should not be required to master them. Having, by this argument and the cumulative effects of other incidents, been drawn actively into the struggle, the writer joined a few of his colleagues from the College of Liberal Arts and Sciences on March 6, 1952, in making presentations at an open meeting of the School Problems Commission of the State of Illinois, which is charged by the General Assembly "to consider and study all germane factors in an effort to determine the improvements necessary to raise the educational standards of the public schools to a desirable level." Although the pros and cons of these various events have been extensively debated in The Scientific Monthly and in Illinois newspapers, there is no evidence of significant progress, and it appears that much more ambitious measures will be necessary.

The protests of the various critics of current educational theories are countered in a variety of ways. Local organizations of parents are stigmatized as pressure groups and as either enemies of

‖ See Note 15 at the end of Scientific Monthly article by Bestor.[4]

the schools or as dupes of such enemies. They are crackpots, malcontents, and reactionaries with a horse-and-buggy philosophy, or else they are motivated solely by a desire to lower taxes, regardless of the effects on the schools. They are also called un-American and enemies of our democracy—which, of course, is being valiantly sustained by the octopus (L. A., pp. 9–12). The possible existence of sinister forces desiring the destruction of our schools lends plausibility to this kind of answer. Some of these charges were advanced, with varying degrees of emphasis and directness, by no less a group than the National Commission for the Defense of Democracy through Education, reporting to its parent group, the National Education Association, at the July 1950 meeting of the latter in St. Louis. As for the scholars and scientists, they are met with the voice of authority. They are, perhaps, reputable specialists in their respective fields of research, but only the specialist in education is authorized to deliver himself on educational subjects. The mathematician, although he may have spent his life as a teacher, is rarely to be consulted on questions in the teaching of mathematics, with regard either to subject matter or to manner of presentation. It is the educationist, however deficient in knowledge of mathematics, who must be the primary arbiter of both questions.

In reply to criticisms of educational practices, one encounters not only the arguments already mentioned, but also a variety of statements, of differing degrees of relevance, tending to distract from the main issues. Thus we are sometimes told, quite truthfully, that, as far back as man can remember, college faculties have been critical of the deficiencies of students coming to them from the secondary schools, the apparent implication being that, because this is a current, though acute, phase of an immemorial dispute, it can be dismissed with a shrug.

Efforts to suppress criticism have been reported from a number of reliable sources, though there has been no noteworthy attempt to discourage us at Illinois from speaking our minds. We have, however, been criticized by colleagues for carrying the argument outside the confines of the university, on the ground that the dispute might prove detrimental to the institution and might undermine confidence in public education. It is indeed generally politic to settle intramural arguments with a minimum of publicity. This is far from being an intramural problem, however; it is a grave national issue, in which industry, the government, and society as a whole all have a vital concern. We could not, if we would, settle it within the confines of any university or all universities, for the tentacles of the octopus are all-embracing.

Symptoms of Progress

In this section, I shall restrict myself to a few symptoms with which I have, in somewhat haphazard fashion, come into contact. The topic is too large to permit an adequate survey on the present occasion. At several successive annual meetings of the Illinois Section of the Mathematical Association, there were items on the program concerned with secondary school mathematics. Before the 1952 meetings, a special committee prepared for a panel discussion and worked up a set of resolutions, one of which (Resolution II) commented on the present poor mathematical preparation of entering college students and put the section on record as approving a general strengthening of mathematics offerings at all precollege levels. Resolution III provided for "a committee of five members of the Illinois Section of the Mathematical Association of America and/or the Illinois Council of Teachers of Mathematics distributed as follows: two classroom teachers of college mathematics, two classroom teachers of secondary school mathematics, and one classroom teacher of elementary school mathematics, which committee shall be known as the Committee for the Strengthening of the Teaching of Mathematics. . . ." The committee was charged with a specified set of appropriate duties. It hopes to begin its work by embarking on a realistic survey of the mathematics actually being taught in the schools of the state.

The College of Engineering of the University of Illinois, handicapped by the poor mathematical preparation of its incoming freshman, took steps leading to the appointment of a committee to study and report on the situation. This committee was composed of engineers, mathematicians, and representatives of the College of Education. As one result of its labors, a pamphlet was produced. *Mathematical Needs of Prospective Students in the University of Illinois College of Engineering*, which is expected to have a strong effect in increasing the number of entering freshmen who can immediately take analytic geometry and can take college physics concurrently with calculus the next semester. The booklet has been widely distributed and has awakened considerable interest in high schools and in other colleges. Incidentally, it has aroused the hopes of some secondary school teachers that they may get something more substantial to teach than at present. Following the production of the pamphlet, there has been a preliminary development of tests to measure the

relevant competencies attained by secondary school students. An experimental high school program implementing the recommendations of the pamphlet has been prepared by a committee of similar composition to the one mentioned above and has been put into at least partial operation at University High School. These things are mentioned to illustrate the possibilities of collaborative effort among mathematicians, educators, engineers, and others.

Earlier in this paper, an article in *Higher Education* was quoted, to the effect that the pattern of high school subjects bears little relationship to success in an engineering college. At the same time, the critical shortage of engineers has led the Engineering Manpower Commission, working on a wide front and operating through high school advisers, to stimulate enrollment in engineering colleges. Given an irrelevant high school program, it is extremely difficult to guess which students should be encouraged toward engineering, with the result that we can expect an unnecessarily large number of misfits. We can only hope that these misfits will discover the mistake before too much of their own lives and the efforts of their teachers has been wasted. With military service just around the corner, a boy can hardly afford to embark on a college curriculum for which he is ill-suited. Aptitude tests are, to be sure, of some assistance in determining which students should be advised to consider scientific careers. A high score on such a test should, however, be regarded with mixed feelings, for it is partly an indication of what *might* have been. It suggests what progress in learning the student could have made, had he only been confronted with appropriate opportunities in an atmosphere conducive to intellectual effort. On the other hand, when it comes to discovering potential scientific talent, a sound mathematical course may well be more effective, and is certainly of far higher value to all concerned than any aptitude tests that the ingenuity of man can devise. The colleges are now generally devoting a year or more of mathematical course work to studies that belong in the high schools. In the case of engineering, where the essential training is largely of a sequential nature, this has the effect of seriously delaying the entire program of studies. It is to be hoped that the Engineering Manpower Commission will be influential in stimulating the strengthening of mathematical offerings in the secondary schools. A counterpart of the Engineering Manpower Commission, the recently organized Scientific Manpower Commission, will presumably bend its efforts in the same direction insofar as training and education in mathematics are concerned.

Proposals for Further Progress

Although the items outlined here are mere symptoms of progress, they may furnish a basis for possible activities in other regions, with such improvements as experience may dictate and ingenuity devise. In general, the writer believes in the feasibility and efficacy of collaborative efforts among college, secondary and elementary teachers, and such professional educators as prove cooperative. The sections of the Mathematical Association of America, the state councils of teachers of mathematics, and various other organizations can render valuable service. In particular, all possible resistance should be offered against the drive to weaken college entrance requirements. We should, on the contrary, strive to build up these requirements toward that point where the colleges will no longer devote a substantial part of their effort to the teaching of high school subjects. As another measure, certain to prove unpopular with those who want freedom to manipulate the entire secondary school program, the writer advocates legal guarantees that every student be provided with specified educational opportunities in the public schools, the specifications to be adopted after careful consultation with all interested groups. The consultation and the groups consulted must not, however, be dominated by professional educators, although the opinions of the latter should be considered along with the rest. Once standards are adopted, they must be implemented, perhaps by testing procedures like the New York Regents Examinations, perhaps with the aid of tests devised by Educational Testing Service, or possibly in some other way.

As a longer-range project, the writer recommends a large-scale study, perhaps sponsored by one of the foundations or by a national agency, directed toward analyzing the general problems of

1. The mathematical competencies considered desirable by colleges of liberal arts, engineering colleges, industrial organizations, and government agencies on the part of high school graduates, both those bound for college and those terminating their formal education with high school.¶
2. The extent to which the schools of the country are meeting these needs.
3. Measures required to bring about any changes necessary to meet such needs.
4. Determination of the actual and desirable lines of demarcation between "high school" and "college" mathematics.

¶ Such work has been done in the past by highly competent groups. It should be carefully reviewed and brought up to date.

Such a project would involve the services of a group of mathematicians and other scientists, statisticians of the highest caliber, professional educators sympathetic to the investigation, representatives of the general public, and representatives of the various groups listed under (1) above. Part of the work would consist in analyzing the validity of statistical arguments employed to justify some of the present practices, and part would consist in planning and conducting surveys to furnish an adequate basis for conclusions and recommendations.

Finally, the writer wishes to express general support for a set of resolutions proposed by his colleague Arthur Bestor to the American Historical Association, and designed to promote a cooperative undertaking by the learned societies in all suitable disciplines. His proposal begins with a preamble expressing alarm at anti-intellectualist tendencies and calling for "cooperation among all the learned societies of the country, acting through an independent, interdisciplinary Commission of their own creation," in activities "designed to uphold and strengthen sound, systematic, disciplined intellectual training in the public schools." A first resolution, in several parts, affirms a belief in certain educational principles. A second resolution "calls upon its [the AHA's] sister learned societies in every field to join with it in creating a Permanent Scientific and Scholarly Commission on Secondary Education," adequately financed and charged with a list of appropriate specified functions. On December 29, 1952, the American Historical Association referred the proposal to a committee for further study.

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SCIENCE ON THE MARCH

RECESSION OF EASTON AND DEMING GLACIERS

Mount Baker, a glacier-crowned volcanic peak, is located in northwestern Washington, about 15 miles south of the international boundary (Fig. 1). Washington's third highest summit, it rises to an elevation of 10,775 feet above sea level and carries at least nine large glaciers, all but one beginning at and radiating outward from the summit. The Easton and Deming glaciers were selected for study primarily because of their close proximity to the Koma Kulshan ranger station where the writer was employed by the United States Forest Service during the 1952 fire season. These glaciers are accessible by 12 miles of graveled road from the town of Concrete to the ranger station, by 10–12 miles of good trail from the station to Easton Glacier, and by 2 miles of cross-country travel from there to Deming Glacier.

Considerable information is available on Easton Glacier, and photographs taken in 1917, 1925, 1931, 1947, and 1952 strikingly show the recession undergone by the glacier. Less information is available on Deming Glacier, but photographs taken in 1931, 1947, and 1952, and information taken from earlier topographic maps provide a fairly accurate record of its recession.

Glacial Features and Nature of the Recession. Easton and Deming glaciers coalesce and form a continuous expanse of ice covering the entire south and southwest sides of the mountain, with but a single rock outcrop near the top marring the glistening whiteness. Only at the lower margin, where ice tongues extend short distances into the valleys, with bare bedrock ridges lying between, is the continuous ice mass separated into 2 distinct glaciers.

An excellent vantage point from which all of Deming Glacier can be seen is the open ridge overlooking the glacier on the south (Fig. 2). By looking in a northeasterly direction to the source of the two glaciers at the crest of Mount Baker, one can see the clean cover of perpetual snow on the upper part; where crevassed, the annual snow increment is exposed in layered bands of varying thickness. Snow and ice accumulating on the summit plateau overflow and descend steeply over the Roman Wall to the upper slopes of the glaciers.

Fig. 1. Topographic map of Mount Baker, surveyed in 1907–08, edition of 1915, U. S. Geological Survey.

Below the wall the crevasses appear, deep wide lateral flexures with ice or snow cliffs on the upper sides.

The ice tongue protruding from the Deming ice front flows into the deep cirque lying at the south base of the Black Buttes. The walls of the cirque are very steep and high, with considerable overhang, and snow avalanching down them provides much additional nourishment to the glacier. In a riverlike manner the glacier swings in a full quarter-circle, flowing in a southerly direction and leaving the cirque by way of a narrow defile descending into a deep canyon. Through the defile the glacier is broken by a spectacular icefall. Here the surface snow is displaced and broken into large blocks, with the blue glacial ice generously exposed. At the base of the icefall where the ice is smoother, Deming Glacier turns and flows in a southwesterly direction. The terminal zone of the glacier is beyond this last turn.

Within the terminal zone Deming Glacier is rapidly wasting, as evidenced by much debris hiding the ice and by the chaotic appearance of the

FIG. 2. Deming Glacier icefall on July 9, 1942. A large meltwater stream flows on bedrock along left side, with the Black Buttes on the skyline and Mount Baker at extreme upper right. Note high ice cliffs just right of center.

FIG. 3. Terminal zone of Deming Glacier showing meltwater stream issuing from the base of the ice. The 1907 position of the ice was at or near the place where the stream leaves the barren glacial trough and turns to the left beyond the line of trees.

surface (Fig. 3). Intersecting ridges and hummocks interspersed with closed depressions, all having considerable relief, form the surface of the glacier. That the surface debris is resting on the ice, closely conforming to surface irregularities, is demonstrated by the fact that where the debris has slumped glacial ice is revealed. Beyond the ice front the debris has been "let down," so to speak, by the melting of the ice beneath it, until now it conforms with the lesser relief of the underlying valley floor. A very milky meltwater stream issues from the base of the ice tongue and enters a narrow V-shaped stream channel about a quarter of a

mile beyond the present terminus. As nearly as can be determined from a distance, the stream has cut its channel about 100 feet into the bouldery valley fill without exposing bedrock.

Except at the terminal zone of both glaciers, meltwater streams are notably absent on the surface. Evidently surface streams are unable to flow even short distances without interruption by deep lateral crevasses. At the base of the Black Buttes, where ice cliffs are numerous, a few large streams emerge, cascading abruptly over rock precipices to disappear again beneath the ice. A large stream emerges from the ice base and flows between the southern wall of the buttes and the ice mass, disappearing beneath the ice after flowing a short distance on bedrock.

The 1907 Mount Baker quadrangle map places the terminus of Deming Glacier fully four fifths mile beyond the present-day terminus, near the place where the meltwater stream leaves the barren glacial trough and enters the old-growth forest. The 1907 terminus was at an elevation of approximately 3800 feet, as compared with a present elevation of about 4600 feet. Figure 4 shows the relative position of the ice about 1931, when the terminus stood 2500–3000 feet behind the 1907 front. The Hamilton and Mount Baker quadrangles (Fig. 5), edition of 1952, show the present-day terminus about four fifths mile behind the 1907 position at the edge of the forest.

Three ice tongues or lobes protrude from the lower margin of Easton Glacier. From their general relation to the surrounding topography, the lower ends of these ice tongues are many hundreds of feet short of the advanced position shown on the 1907 map. Although the two eastern lobes were not visited, they were viewed from a distance, and their earlier positions can be estimated with rea-

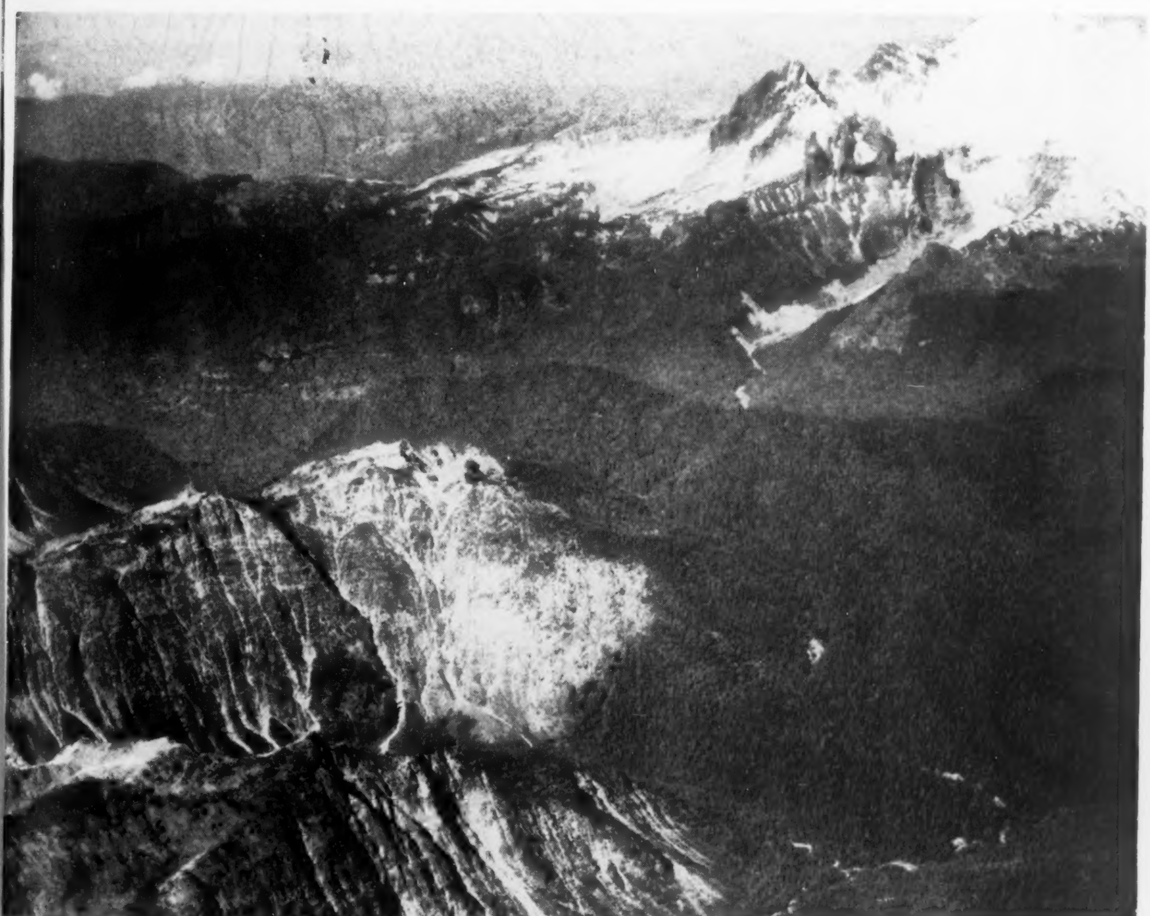

FIG. 4. Aerial photograph, about 1931, showing Mount Baker from the southwest. Deming Glacier occupies trough at center and flows toward observer. The 1907 position of the ice was at extreme lower end of glacial trough at edge of forest. Note relatively clean surface of ice at terminus and compare it with debris-laden ice in Figure 3. (Photo by U. S. Forest Service.)

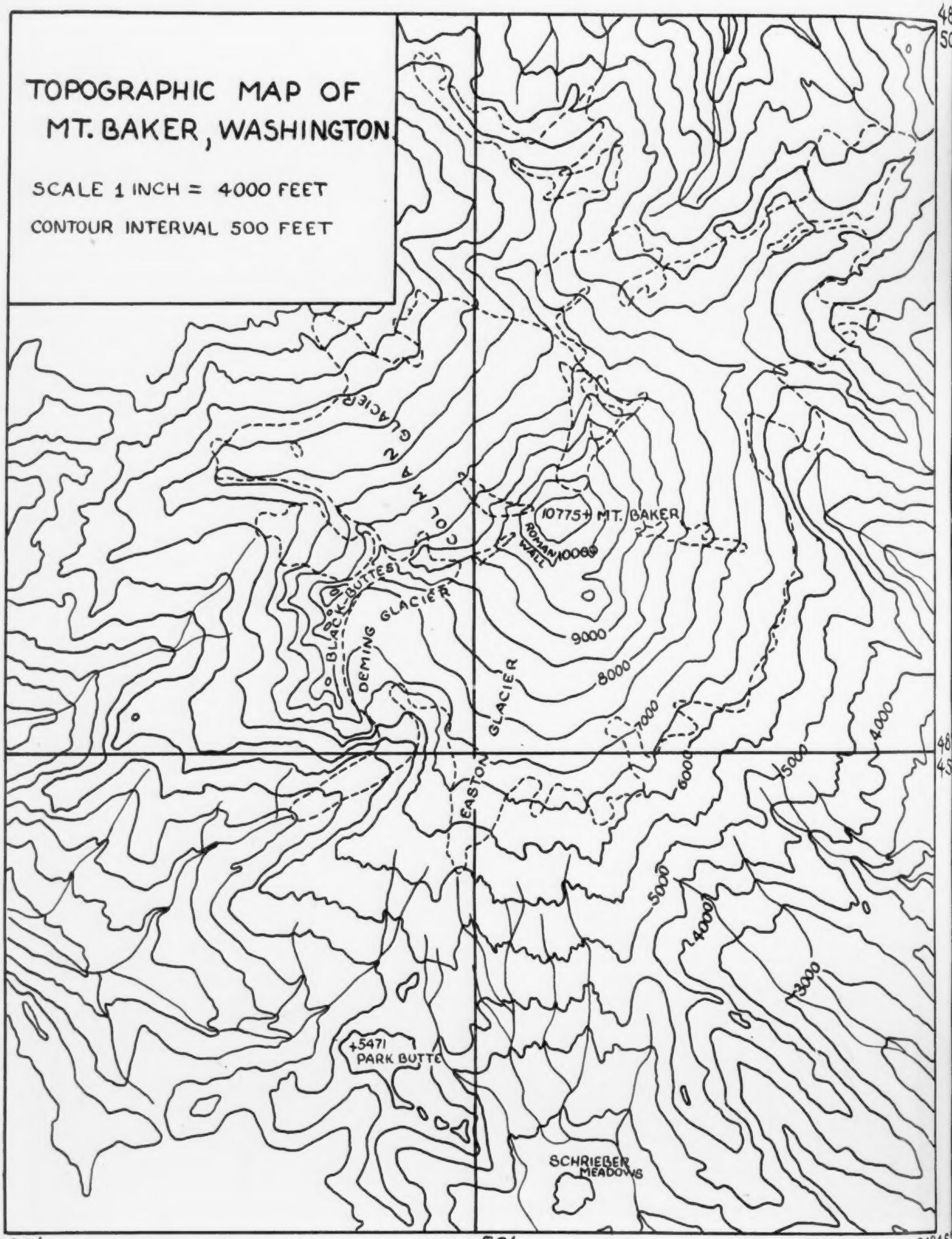

FIG. 5. Topographic map of Mount Baker, redrawn from Hamilton and Mount Baker quadrangles, edition of 1952, U. S. Geological Survey.

Fig. 6. Huge morainal embankments were deposited by aston Glacier when the ice occupied the barren glacial rough. Mount Baker in the background.

Fig. 7. Easton Glacier and Mount Baker, showing advanced position of the ice in 1917. Judging from the size of the glacier at right margin of picture, with the ice at least 250 feet thick at this point, as determined by estimate and by later pictures, the terminus is considerably less than 1000 feet from the 1907 position. When compared with later pictures, both the retreat and downward wasting of the ice are very obvious. Orange-painted arrow marking 1934 terminus is on rock outcrop just to left of meltwater stream shown at center. The right margin of the glacier almost exactly coincides with the location of the marker. (Photo by George Ely.)

onable accuracy from the map and from early hotographs. The detail and the nature of the ecession of Easton Glacier are best shown by the ongest, or western, ice tongue, since this tongue as left a fairly complete record of its recession.

As indicated by the great quantities of rock lebris forming huge lateral embankments (Fig. 6), he recession of the west tongue must have been alanced by its forward movement for many years. The lateral ridges extend unbroken from behind he present ice front a distance of about $1\frac{1}{2}$ miles, urving gradually inward near the apex and decending to a low end moraine, which is breached t the center by the meltwater stream. The end oraine marks the approximate position of the e tongue in 1907. The crests of the lateral moaines are sharp and narrow, overhanging and lumping very readily on the inner, or trough, ide. Huge boulders continually break loose from he trough sides of the laterals and fall to the floor of the glacial trough. Plumes of whitish dust rising from the trough mark the paths of many small rock slides started by these tumbling boulders. On

Fig. 8. Mount Baker from Park Butte, about 1925, showing Deming Glacier and the Black Buttes at left, and Easton Glacier on right. (Photo by George Ely.)

FIG. 9. Aerial photograph, about 1931, showing Mount Baker from the south. Deming Glacier and Black Buttes are at left, and Easton Glacier is at center, occupying upper end of barren glacial trough. Dotted lines show relative positions of the ice in 1907, 1917, and 1925. "X" marks the place where the Mountaineers painted the orange arrow showing the 1934 position of the ice. Note debris-covered terminus of Easton Glacier. (Photo by U. S. Forest Service.)

the ridge side, however, the bouldery debris is firm held in place by soil, alpine plants, and trees.

The 1907 map of the Mount Baker quadrang shows the snout of the west lobe of Easton Glaci about 1 mile northwest of Schrieber Meadows an elevation of about 4100 feet. The writer visite several old-timers in the vicinity who vouched f the fact that the glacier occupied this advance position near the turn of the present century. Th Hamilton quadrangle map, made by multiple methods from 1947 aerial photographs, shows th terminus at an elevation of about 5450 feet, ful $1\frac{1}{3}$ miles behind the 1907 position at the low end of the glacial trough.

The earliest photograph shows Easton Glaci in 1917 (Fig. 7). At that time the snout ende only a short distance—perhaps not more than 400 600 feet—behind the 1907 position. Since the however, the entire ice tongue extending across th base of the view has completely disappeared, ar the amount of thinning at the head of the trou has been appreciable.

The condition of the ice in 1925 after conside

FIG. 10. Showing thin, debris-covered terminus of Easton Glacier on August 11, 1952.

le recession had occurred is shown in Figure 8. he terminus stood at an elevation of about 4800 et and had receded almost half the distance from e 1907 position to the present-day front at the pper end of the glacial trough. Figure 9, an aerial otograph taken about 1931, shows Easton Glacier ter it had receded more than ¾ mile from the 07 front.

Panoramic photographs, taken in September 35 by the United States Forest Service from ck Butte fire lookout house, place the terminus an elevation of about 5200 feet, or the same elevation as the lookout house. This elevation can be tablished with considerable accuracy, since a line nnecting points of elevation equal to the height Dock Butte almost exactly coincides with the out of the glacier at the lowest point.

The present-day terminus is several hundred feet hind the 1947 position. Near the terminus the ice ws outward and descends steeply for 300 feet, en gradually bows inward, thinning and narrowg into a very short tongue extending only a few ns of feet before it is completely dissipated by lting (Fig. 10). This short ice tongue has inned to such an extent that it has almost comletely disappeared. Large chunks of the tongue main, but they are detached from the main mass d are melting very rapidly. Much sand, silt, and ouldery debris cover the isolated masses of ice, d small meltwater streams are prevalent on the e surface. On the east side a narrow section of e still is attached to the main glacier, but it is ery thin. Thinning is hastening the rate of recesion, as evidenced by the condition near the terminus where large sections stagnate and simply waste way. The Hamilton quadrangle map and the photographs from which it was made show a much longer ice tongue extending beyond the Easton ice front. Assuming the farthest or lowest isolated ice chunk to be at or near the 1947 front, the writer on August 11, 1952, measured by pacing and found the distance from the present ice front to the lowest chunk to be 550–600 feet. Recession from 1947 to the present is about 600 feet.

Records kept by the Mountaineers, of Seattle, are shown in Table 1.

TABLE 1*

MEASUREMENTS OF RECESSION OF EASTON GLACIER

	Recession (Feet)
1934–35	190
1935–36	170
1936–37	116
1937–40	429
6-year average	905

* From 1934 until 1937 the Mountaineers measured the annual recession of Easton Glacier on Mount Baker. However, during the years 1938 and 1939 no measurements were taken. Therefore, the figures acquired this year must be made into a 3-year average.
The terminus of the Easton Glacier forms two thin tongues of ice, of which the eastern one is the longer. Because of the small amount of ice in these tongues recession may be quite rapid during the next few years. The Easton Glacier was measured October 13, 1940, by Fred Becky and Paul and Ed Kennedy.

On August 20, 1952, the writer and Ben Carey, of Mount Vernon, Washington, measured the total recession from the orange-painted marker made by the Mountaineers at the 1934 terminus and found it to be 2210 feet. These figures show that in the past eighteen years the ice has been retreating at an average rate of about 123 feet a year.

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ANIMISTIC THINKING AMONG COLLEGE AND UNIVERSITY STUDENTS

THERE is evidence that a large percentage of children, up to at least twelve years of age, believe that many objects, classified by iologists as inanimate, are alive. The data conerning animism in children come from groups as iverse as the Swiss,[1] white American groups,[2] and merican Indian groups.[3, 4] These investigations ndicate also that children who believe objects to e living frequently attribute to them various forms f consciousness.[3, 5] In other words, the child atributes biological and psychological characteristics to entities which the scientist classifies as inorganic matter.

Piaget,[1] who is largely responsible for the modern interest in animistic thinking in the child, proposed that in civilized man it disappears at about age twelve or shortly thereafter. Adults, he claimed, believe that only plants and animals are living. But it does not appear that Piaget has questioned adults on this point, nor, until recently, have other investigators directed their inquiries to adult subjects.

In 1949 Mallinger and I[6] reported the results of applying the techniques that have been used with children to 36 persons aged seventy and over. We found that 75 per cent of these subjects attributed life to some objects other than plants and animals. Although it was suggested that the high percentage of older persons exhibiting animism might be due to mental deterioration accompanying age, it was kept in mind that no data on adults in early maturity were available.

Recently I have undertaken the questioning of various groups of younger adults. I propose eventually to interrogate persons of average intelligence and education, but pilot studies were conducted with college and university students. The results of the investigations have proved so surprising, not only to me but also to several groups of psychologists to whom they have been reported, that it seems advisable to present a brief preliminary report.

In studies of children, the method was that of an individual interview.[7] With college and university students individual questioning did not seem necessary, and a group method was used.

The instructions were as follows:

> I am going to ask you questions about some common objects. You may think some of the questions very easy; some may be rather hard. I am asking these questions of many kinds of people. Both the easy and the hard questions are asked of you because I want to know how persons of all sorts answer the very same questions. Please answer each question seriously, though some questions may seem very simple.

The subjects were then shown, or reference was made to, several objects. With regard to each object, the subjects were to write down whether it was living or not living, and were required to state the reason for the answer. The answer regarding one object was written before the next object was presented.

The first group to be questioned was a class in child psychology which had not studied or discussed animism. The students were enrolled in a large private university in New York City, and were primarily graduate students of education. Of the 67 students, 54 were graduate students, 15 of whom already held a master's degree; 45 had had teaching experience. The objects about which they were questioned were an unlighted match held before them, the same match lighted, an electric clock on the wall of the classroom, the sun, the wind, a five-cent piece, a pearl, gasoline, and the ocean.

Forty-five per cent of these subjects stated that one or more of these objects were living. The objects most often called living were the lighted match, the sun, and the ocean. In the case of each of these, approximately one third of the group gave animistic answers.

The questions were next asked of a class of 71 students in introductory psychology at one of the city colleges of New York. Most of the students were sophomores. The objects employed were identical with those presented to the graduate students of education, except that the nickel and the pearl were dropped and the term "clouds" was added. Thirty-seven per cent of these students gave one or more animistic answers.

Subsequently, a similar set of questions was given to a mixed group of 34 graduate and undergraduate students enrolled in child psychology in the summer session of a Southwestern university. The terms employed with this class were the sun, clouds, sea, lightning, wind, stars, and earth. The percentage attributing life to one or more of these items was 48.

In the light of these findings, it seemed worthwhile to make a serious attempt to find college students who were not animistic. The best available candidates seemed to be 68 sophomores in the city college previously referred to who were just completing the third semester of an integrated science course. Most of the work of the current semester had been biological, with a strong emphasis upon the properties of protoplasm and the distinctive characteristics of living things. They had had no other science course in college. The list of terms presented to the introductory psychology students was repeated with this group. On the basis of the results obtained in this instance, my belief in the possible efficacy of instruction in biology was somewhat restored. Only 12 per cent of these students attributed life to any of the objects about which they were questioned.

On several occasions, the presentation of the results described here has caused my auditors to be rather incredulous. They insist that, other than the science students, the subjects must not really have meant what they said. It was suggested that they were being poetic, or philosophical, or whimsical. The effective answer has been to read the detailed animistic responses and to ask whether the answers can correctly be considered poetry, philosophy, whimsey, or science. Space permits the printing of only a few examples, illustrating the naïve character of most replies. The following are typical:

The lighted match: "Living, because it has flames which indicate life." "Living because it is

burning brightly, giving forth something." "Dying —I saw it being burned."

The sun: "Living because it gives forth energy. Gives us power, warmth, light, and energy. Makes things—living things—thrive and exist." "Living because it gives off heat." "Yes! Living! Without breath, but living, scientifically living, changing."

The ocean: "Living because it is constantly maintaining life. Movement is characteristic of it, and life is brought forth by it." "Living. It has moods and is temperamental just like many human beings." "Living—it moves and makes noise and is powerful and changing. Sometimes calm, sometimes stormy. We cannot control it." "Living, continually in motion, changing, etc."

These answers are equivalent to those earlier recorded for children, except that some of the adult answers reflect a larger vocabulary. The reasons offered by the adults for their answers belong to the classes which Piaget has called Stage II and Stage III types; that is, an object is said to live because it moves or is active in some way (Stage II) or because it is self-moved (Stage III).

But did these students merely use the word "living" as equivalent to "active" or "moving," or did they, as children do, attribute other traits to these "living" objects? A few questions were asked to attempt to get at this matter. With the graduate students of education, after the questions previously reviewed had been answered, they were immediately asked the following additional questions:

"Many ships are lost at the bottom of the sea. We cannot find them. Do you think the sea itself knows where they are?"

"This pearl was once in a shell in the sea. When the water moved, could the pearl feel the movement of the water?"

Of the students who had given one or more animistic answers to the earlier questions, one third attributed consciousness to the sea or to the pearl or to both. The answers regarding the sea were as follows:

"Yes, the chemicals in the sea come in contact with sunken vessels and are aware." "Yes, the sea does know the location of the lost ships because they are in the bottom of the sea." "Yes, if it [the sea] is living, it ought to." "Yes, the sea rubs over the lost ships and knows them to be there." "No, the sea doesn't care to know. There are too many of them. The sea *could* know if it wanted to."

In regard to the pearl, the following replies were obtained:

"Probably as much as a very young fetus might feel the effect of water in the mother's womb." "Yes, the pearl was part of a living thing." "Yes, through the living oyster." "Yes, because it is changed by friction."

The introductory psychology students also were asked the question about the sunken ships. In addition, the following question was asked: "The tides are caused by the pull of the moon upon the ocean. Do you think the ocean can feel the pull of the moon which causes high tides?"

Of the 12 college students who had said the sea was living, five attributed consciousness to the sea in reply to one of or both these questions. Their responses were similar to those already quoted.

Obviously much more extensive investigations of the attribution of consciousness to objects lacking a nervous system are called for, but the data just presented demonstrate clearly that such anthropomorphism is not limited to children.

One would not be surpised if ideas such as those just described were found in a primitive or a backward group (I have data showing that in some little-educated groups the percentage animating the sun and other natural objects is close to 100). But, in my experience, few psychologists except to find such ideas among teachers and college sophomores. It should be borne in mind that teachers and sophomores often have had no more specific instruction than primitive peoples concerning the distinction between the animate and the inanimate and concerning the dependence of consciousness upon a nervous system. Apparently, in the absence of specific instruction, "educated" persons in modern societies possess many conceptions of the world that are identical with those of the child and of the uneducated. A fuller discussion of the importance of this fact must await a later occasion. Meanwhile it is hoped that others will avail themselves of opportunities to verify, expand, and refine the observations reported here.

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A POSSIBLE IMPLICATION OF THE DUNE TOPOGRAPHY OF THE SOUTHERN COLORADO PIEDMONT*

AMONG the more outstanding topographic features in the foothills of the Colorado Front Range are the excellently developed pediments. Those lying to the west of Fountain Creek (Fig. 1) have been well described by B. A. Tator in recent volumes of the *Bulletin of the Geological Society of America* (Vols. 60, 62).

It has been my privilege to work with Tator in his research on the mountain-front pediments over a considerable number of years, and I am essentially in agreement with his conclusions as to how they were formed. They are fairly extensive, remarkably flat, bedrock surfaces (Fig. 2), which truncate the upturned sediments with seeming disregard for the varying resistance to erosion one expects from rocks with differing lithologic characteristics. The slope of the surfaces compares favorably with the gradients of the associated streams—slopes up to 200 feet per mile not being uncommon. Usually the pediments are covered with a few feet of alluvium, which in bouldery character and granitic composition also corresponds fairly well with the material carried by the streams of the area today.

Tator explains the pediments as being the erosional product of intermittent streams in a semiarid region. Very briefly, the process calls for a shifting of the stream channel as a result of self-plugging by debris left by the heavily laden and turbulent flow during and after a storm, and side-wall retreat produced by weathering and attendant downslope mass movement. The formation of surfaces at more than one general level is explained as the result of climatic change that prolonged stream flow and increased downcutting during a subhumid cycle between two periods in which semiaridity provided optimum conditions for pedimentation.

During the past few years our attention was attracted to, and research was started on, similar surfaces that lie to the east of Fountain Creek. I have undertaken the major portion of the research there and originally I had the idea of turning out a supplement to Tator's work. It was our belief at first that the mountain-front pediments and those east of Fountain Creek were probably formed by similar processes; therefore, my work was to be primarily concerned with delimiting the surfaces and determining their sequence of development. I have found the work much more involved than that, however. There are, of course, similarities between the surfaces of the two areas, but also there are what currently seem to be important differences.

Surfaces East of Fountain Creek. Even a cursory study of the excellent new 7½ minute quadrangle maps (Pike View, Falcon, Falcon N.E., Colorado Springs, Elsmere, Corrall Bluffs, Fountain, Fountain N.E., Fountain S.E., Buttes) of the area east of Fountain Creek will reveal some facts about the pediments. In the first place, they are much better preserved than the mountain-front pediments and so, at least, appear to be more extensive. Second, they can readily be divided into two groups. One group, which is obviously younger, slopes to the southwest and is apparently controlled by the present drainage line of Fountain Creek. The older group slopes to the southeast and is graded to drainage lines that lie at some distance to the east of the modern course of Fountain Creek, which carries the mountain-born drainage to the south.

Early field work revealed other differences and at least one similarity between the pediments of the two areas. The younger slopes to the east differ from the mountain-front pediments in the nature of their alluvial cover and in their direction of slope. The alluvial cover of these southwestward slopes is a sandy gravel ranging in size up to pebbles, as compared to the bouldery gravels of the southeastward sloping mountain-front pediments. They are like the younger mountain-front pediments in that they are both graded to Fountain Creek. The older surfaces east of Fountain Creek may actually be remnants of the older mountain-front pediments. Their slope is in the same southeasterly direction, and their alluvial cover also contains boulders of ancient crystalline rock. The most significant difference between these older eastern surfaces and their mountain-front counterparts is the dune topography that modifies large expanses. The dunes were first detected on aerial photographs and later verified in the field (Fig. 3). So far as is known there are no dunes west of Fountain Creek.

Dunes. The full extent of the dune topography has not yet been determined (Fig. 1); however, it is known to reach from the areas described by Gilbert and by Fisher (east and north of Pueblo) north to the Black Forest (which lies on the divide between the South Platte and the Arkansas), the

* Based on a paper presented in the joint session of AAAS Section E and the Geological Society of America during the Annual AAAS Meeting, St. Louis, Missouri, December 26–31, 1952.

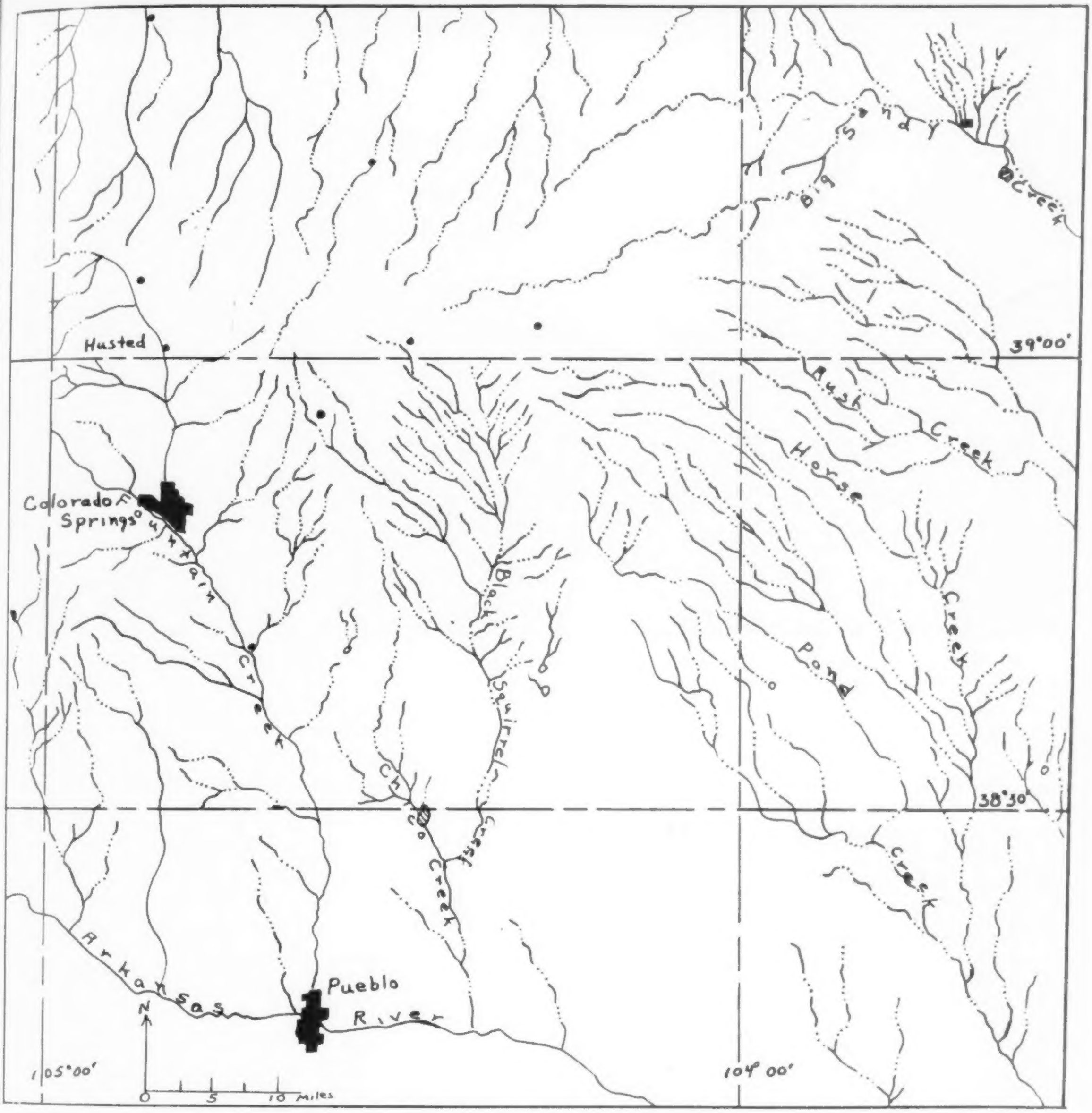

FIG. 1. Drainage map of eastern El Paso County, Colorado.

southern boundary of which it skirts westward to the vicinity of Husted. It is well developed in the northeast environs of Colorado Springs, whence the western limit swings south and east, coinciding to a marked degree with the divide between the eastward-sloping older surfaces and the younger ones that slope to the west. These tentative boundaries enclose an area of more than 1400 square miles, for the most part characterized by extensive flat surfaces the topography of which is nearly featureless, except as it is modified by the younger dune topography and the main drainage courses.

That there are no dunes west of Fountain Creek, even though there are surface remnants there as old as some of those covered by sand to the east, is probably best explained by the fact that the mountain mass undoubtedly received sufficient moisture even during more arid times immediately to the east to support vegetation similar to what it has today. In addition, the prevailing winds were apparently from the northwest, as shown by the orientation of the transverse dunes (Fig. 3), and so they could not have delivered sand to the mountain front from the desertlike piedmont to the east, where dunes were actively forming.

The recently published map of eolian deposits,

FIG. 2. View to east across a segment of one of the older mountain-front pediments, showing truncation of upturned beds.

and reports dating as far back as those of G. K. Gilbert (1896) and C. A. Fisher (1906), and as recent as F. A. Melton's "Southern High Plains" paper (1940), prove that the existence of dunes in the Colorado Piedmont and on the High Plains has been long and widely known. With the exception of Melton's paper, however, these reports have been concerned with the existence of active dunes, whereas those on the eastern pediments, like those on the Southern High Plains, are stabilized by vegetation and appear to have been so for a considerable period of time.

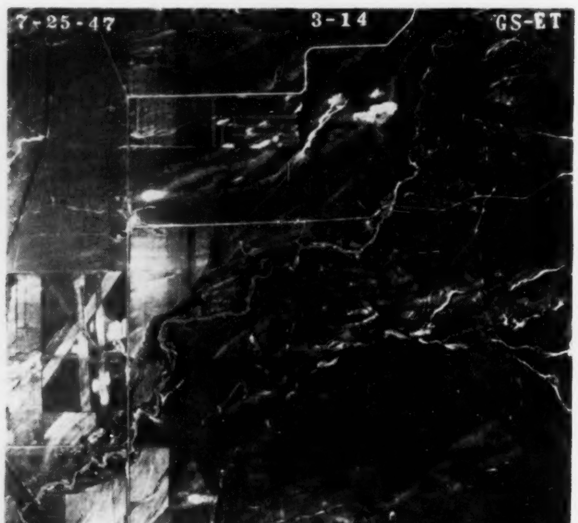

FIG. 3. Vertical view of area approximately 9 square miles, showing well-developed ancient transverse dunes trending about N 45° E. Very light areas are modern blowouts on lee side of old dunes, indicating a radical change in direction of effective wind. (Used by permission of U. S. Geological Survey.)

The dunes are unevenly distributed and diversified in size, ranging from small longitudinal and transverse types that cover less than a few hundred square feet to longitudinal ridges that are several miles long and over 30 feet high (Fig. 4). In numbers they range from dune complexes, which completely blanket areas of many square miles, to areas of comparable dimensions that are almost completely devoid of dunes.

The variation in particle size in the sands of different dune areas seems to be related to the sources of the materials. From the few samples that have been studied, it appears that there are two major sources for the sand. On the surfaces underlain by the Pierre shale, the sand is rather well sorted and fine-grained, as shown by the curve on the right side of Figure 5, whereas the dunes found on the surfaces underlain by the Fox Hills and Laramie formations, and particularly by the Dawson, are composed of poorly sorted sand with

particles up to fine gravel in size, as shown by the other curves in Figure 5. This pronounced difference is thought to have resulted from the fact that the fine-grained, well-sorted sands were derived for the most part from sand-filled, intermittent stream courses, whereas the relatively coarse-grained and poorly sorted material was to a larger extent derived locally from the underlying residual mantle. This would be especially true of the rather extensive areas in the north, which are underlain by the poorly cemented Dawson arkose.

Relation of the Dunes to Pedimentation. Although much additional work needs to be done before the exact relationship of the dunes to the pediments can be established, every dune thus far examined is resting on the pediment or on the relatively thin mantle of alluvium that usually covers the pediment. It seems quite probable, therefore, that whatever circumstances were responsible for the formation of the dunes undoubtedly also played an important role in the history of the surfaces. The abundance of dunes on at least portions of all the southeastward sloping surfaces, in contrast with their complete absence on similar but younger surfaces that slope to the southwest, implies a climatic change to desert conditions between two stages of surface cutting. This implication is further strengthened by the fact that, with only a few exceptions, the dunes of the area today are stabilized by a vegetative cover that supports large-scale cattle raising, whereas within the dunes very little in the way of plant remains has been found.

If we accept Tator's conclusions that the pediments were cut under conditions of semiaridity, and that dissection occurred during more humid times, then the presence of the dunes in the eastern area takes on added significance and perhaps offers added weight to his concept. The dunes seem to have developed between two periods of surface cutting and thus would indicate a climatic change of sufficient magnitude to have interrupted any pediment development in progress at the time of the change. If the pediments were cut during the optimum semiarid conditions, and if a climatic shift toward more humid conditions was accompanied by dissection of the pediments, then, logically, a change from semiaridity to aridity would have resulted in excessive alluviation on the pediments, or even the development of sand dunes.

The validity of Tator's concept depends in large part on the demonstrated fact that the alluvial cover on the pediments is rarely thicker than the depth of scour of the streams that formed them; however, he reports some instances of alluviation in excess of 100 feet on the older mountain-front

Fig. 4. Vertical view of an area approximately 9 square miles, showing the complex nature of dune types and distribution. (Used by permission of U. S. Geological Survey.)

pediments. He recognizes that there is a planate bedrock surface beneath the gravels and that the gravels are water-deposited. Their thickness may possibly be the result of a decrease in gradient of the heavily laden mountain-born streams, which would be forced to drop much of their load on the pediment. However, a re-examination of these older mountain-front surfaces designed to determine their sequence and to correlate them with the dune-modified eastern surfaces, suggests another explanation of the varying thickness of the alluvium.

Increasing aridity in the area in general would not necessarily mean a decrease in the transporting

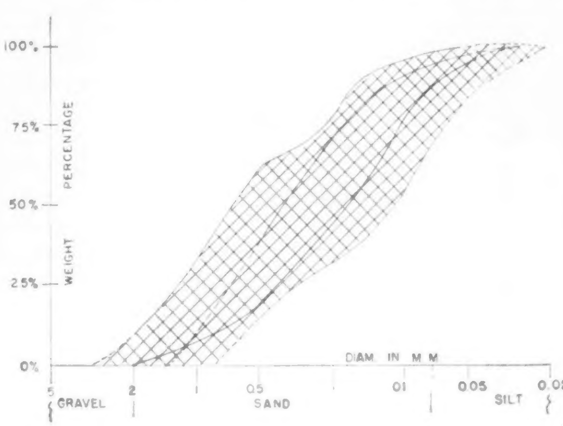

Fig. 5. Cumulative curves plotted to show wide range in sorting and size of material found in the dunes.

ability of the permanent mountain streams, which rise in the snowfields high above the piedmont. On the other hand, it could very well mean a decrease in the incidence of storms along the mountain front and in the piedmont. Since these storms are almost the sole source of water for the intermittent streams that move the mountain-born alluvium across the pediment, any prolonged diminution in volume should result in an accumulation of alluvium on the pediment. Thus, the increase of aridity as implied by the dunes to the east of the mountain front might well have stopped pedimentation and initiated alluviation on the newly formed pediments along the mountain front.

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PHILOSOPHER OF AMHERST

She scrutinized existence,
  she turned life inside out;
hers was an incredulity
  that even doubted doubt.

She drew no swift conclusions
  but with Socratic mind
held counsel with a butterfly
  and interviewed a wind.

She held a seminar for dawn
  to probe time's mysteries;
she cross-examined katydids,
  held inquest for the bees,

then waited for the hemlock
  that would at last disclose
the secrets she could not extract
  from butterfly or rose.

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BOOK REVIEWS

WHEELS WITHIN WHEELS

Flying Saucers. Donald H. Menzel. Cambridge, Mass.: Harvard University Press, 1953. 319 pp. $4.75.

EVERY reader should hail this excellent evaluation of "flying saucer" tales as a distinct service both to science and to the lay public. It was painstakingly prepared for thought-provoking but easy reading. It does not assume any background of scientific knowledge on the part of the reader, and will even help him acquire an ability to weigh evidence and reach scientific conclusions. Perhaps its most useful passages are those explaining scientific methods of approaching unusual physical problems, together with others designed to give every man the ability to safeguard himself against being deluded by fiction masquerading as truth.

Pretenders to scientific knowledge seem to assail the gullible at every turn, sometimes merely to get a tall story across, but often to gain business advantages over a competitor. As the Harvard astrophysicist points out, "Pure-food and narcotic acts protect us from potentially dangerous medicines, foods, or drugs. Yet exploitation of the minds of the American public, feeding them fiction in the guise of fact under the protection of a free press, or frightening people with fanciful ghosts—these, too, are potentially dangerous." Those who cannot spend years of study in theoretical physics, astronomy, or biochemistry do have a means of protecting themselves against the modern deluge of humbuggery. They can scrutinize the source and authority of the evidence. They can test details, look for false premises, gaps, and illogical conclusions.

"With a little thought," says Dr. Menzel, "we can detect fallacious logic, even when it is disguised with scientific vocabulary. Basically, the reasoning of much pseudo science is like the example of the table that turned into a dog: A dog has four legs; the table has four legs; therefore the table is a dog." Or another argument once popularly accepted: "Animals have legs and muscles. Animals can move. The earth has no legs or muscles. Therefore it must be fixed in space."

Just how would the "scientific detective" go to work on flying saucer stories? How can one explain in terms of reality tales that begin, "I saw—I heard"? For example, Dr. Menzel cites the unusual story originating on the campus of the University of Denver, about "men" from space. A Mr. George Koehler introduced a Mr. Newton, supposedly an authority on flying saucers, to the instructor of a class in general science. Newton told a fantastic story (the heart of a later book by Frank Scully entitled *Behind the Flying Saucers*). Widely reported in the press, a wave of incredulous acceptance swept over much of the country. The speaker seemed sincere and had a certain air of technical authority. He told of government attempts to keep secret the arrival of saucer-borne creatures from another planet; miniature men scarcely 44 inches tall, beardless, dressed in 1890-style clothes, of a fiber that could not be identified; wearing shoes of a material resembling human skin. One ship had supposedly crashed near Aztec, New Mexico, another near Durango, Colorado. Dr. Menzel characterized the reported technical description and operation of the saucers as mere "scientific double talk, of no significance at all except to give the impression of authority."

Although some educators have criticized the university for allowing such a lecture to be given, especially by a speaker who attempted to keep his identity a secret, it seems to have served the useful purpose the instructor hoped for. It gave students an opportunity to test basic rules of scientific discipline, to determine for themselves how much credence such testimony was worth. Eight classes valued the lecture at zero in terms of acceptable authentic information, but gave it a very high score as entertainment.

Professor Menzel devotes the major part of his book to denouncing what he calls the "cult of the flying saucers." Saucers are real, he says, in the sense that people have seen something. But they did not see what they thought they saw. Saucers have no connection with atomic experiments; they are not devices invented by Russia; nor are they interplanetary space ships.

On the positive side, he lists true saucer sightings in recent years as

> reflections from material objects: distant planes, jet aircraft, vapor trails, miscellaneous balloons, newspapers, kites, birds, peculiar clouds, spider webs, insects, feathers, etc. Searchlights playing on thin layers of cloud or mist account for many. Venus, Jupiter, various stars, bright fireballs, and even the moon shining through broken clouds have been identified frequently as flying saucers.

Some 20 per cent of sightings that do not fit in these categories are placed in the rags and tags of meteorological optics": mirages, reflections in mist, refractions and reflections by ice crystals. Some phenomena are probably related to auroras, others to unusual forms of shooting stars. The book covers just about all the reports in modern times and follows the trail of unusual visions back through history to early Biblical days. The visions of Ezekiel and Jeremiah, tales of the Flying Dutchman, the books of Charles Fort, Dr. Gee, Lubbock lights, Project Saucer, Specter of the Brocken, temperature inversion, unknown lights of Japan, and the Galloping Ghosts of Nansie Shoto—all receive attention.

A tremendous amount of work went into the preparation of this book. Dr. Menzel should receive the grati-

tude of scientists and laymen alike for his ability and his willingness to shed light on a perplexing, even frightening subject, one that is only remotely allied to his real interest—astronomy.

Of course there are flying saucers—as real as flying dragons, wheels, chariots of fire, flaming cherubims, angels, and rainbows, too! We learn slowly that things are not always what they seem. The search for truth must go on continually, else we could only despair with Sir Alfred Lyall:

> "I think till I'm weary of thinking,"
> Said the sad-eyed Hindu King,
> "And I see but shadows around me,
> Illusion in everything."

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HARVEY'S CLASSIC

The Genera of South African Flowering Plants. Botanical Survey Memoir No. 25. E. Percy Phillips. Pretoria: Division of Botany and Plant Pathology, Department of Agriculture, Union of South Africa, 1952. iv + 923 pp. £2.

IN 1868 the second edition of William Harvey's classic *Genera of South African Plants* appeared. The Botanical Survey Committee of the Union of South Africa entrusted to Dr. Phillips the difficult task of preparing an up-to-date manual that would be a successor to Harvey's *Genera*. This work appeared in 1926, spanning fifty-eight years of great activity and ever-widening knowledge of the new lands that were to be Natal, Transvaal, and South West Africa. Botanical research and discovery in these territories and the Cape Province revealed many new genera and the necessity for revision. Dr. Phillips' second edition successfully links the past twenty-five years of botanical investigations into one comprehensive volume "within the means of the ordinary student of our native flora." Dr. Phillips' colleagues may well greet this work as a sterling contribution to South African biological science; students of the South African flora will find it an indispensable manual. It marks the industry, skill, and learning of one who has devoted all of a professionally active life, and the greater part of an officially retired period, to the perfection of his labor.

The 1926 edition was largely a compilation of 1645 genera gathered from the available literature; this was an increase of 459 genera over William Harvey's list of 1868. The 1951 edition has been enriched by an additional 142 described genera. With the exception of the group of genera related to *Mesembryanthemum*, all the genera cited have been personally examined and described by the author, to which he has also added new generic records and annotations. He follows the system of de Dalla Torre and Harms, which is the arrangement followed in the National Herbarium; and where authorities differ in the placing of genera, the author has properly entered appropriate comments. The first edition contains a synopsis of Engler's system of classification; this has been omitted from the second edition.

The key to the families has been increased by 1[illegible] pages, which is partially accounted for by slightly larger type, but there are many changes aimed primarily at simplification. In preparing the keys to both families and genera, emphasis has been placed upon characters visible to the naked eye wherever possible. As in the 1926 edition, the keys are artificial, and stress has been given to characters exhibited only in the South African plants. Thus the manual is specifically designed for the needs and encouragement of South African students. This is clearly brought out in the introduction, which consists of three pages of instructions addressed to "Any student who has a keen desire to become acquainted with the flora of his district. . ." It includes concise information on the correct procedures for collecting and maintaining a herbarium, the use of the keys, and the method for examining both fresh and dried plant material. (So few of our master works begin on such a practical and elementary level.) The introduction is followed by 44 pages of "Key to the Families" and 806 pages of text, which are generously printed with clear type and wide margins, adding much to the appearance. A bibliography and index of 66 pages complete the work.

Three especially valuable features of this edition are the references to the original publication of the genus, the citation of the type species where possible, and the excellent bibliography of 48 pages. The bibliography is arranged alphabetically by genera, so that the needs of the moment can be quickly met without the distraction of extraneous material. It also includes in its alphabetical arrangement three nongeneric items that would be quickly revealed through use but could easily be missed by the casual reader; these are the related subjects on nomenclature, taxonomy, and floras. Under "Floras" alone, some 56 works are cited, including publications of 1951. The bibliographic analysis completed in connection with this manual is in itself impressive.

It would be presumptuous to criticize a contribution that so sincerely and effectively develops the author's intent. The area covered is large by any measure and exhibits an extraordinarily high endemism distributed over varied and extreme habitats. There are, unfortunately, no references to geographic categories, but many tantalizing and exotic place names that could not possibly appear on a simple map. Nevertheless, one humble suggestion is offered on behalf of outlanders: the inclusion of a map indicating the more important subdivisions of the area treated, which could perhaps serve as end papers in the volume. This manual is certain to become an integral part of any working library of botanists and students of world floras.

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PHILOSOPHICAL ANALYSIS

Ontology. James K. Feibleman. Baltimore: Johns Hopkins Press; London: Geoffrey Cumberlege, Oxford University Press, 1951. xix + 807 pp. $9.75.

IN THIS treatise Professor Feibleman presents what he terms "axiological realism" (*A–R* system) as a finite ontology. There are, according to his account, three possible philosophical positions:

1. Idealism: the universe of essence is alone real;
2. Nominalism: the universe of existence is alone real: as (*a*) subjective idealism: the knowing subject alone is real: or (*b*) materialism: the object of knowledge alone is real: and
3. Realism: both the universe of essence and the universe of existence are equally real.

"Reality" means, ontologically, "having independent being" and, epistemologically, "having identification as the object of that which is true." "Being" (essence) means "the power to affect or be affected."

In the opening section an attempt is made to apply this classification to the history of philosophy, with in most instances resulting distortion of the systems referred to. It is true, indeed, that the justification for the system is not sought in history but in the analysis offered. Nevertheless, the author states that he hopes he has included what is significant in each school, including, of course, contemporary movements.

The system subdivides into (1) the universe of essence, (2) the universe of existence, (3) the subordinate universe of destiny.

The universe of essence is the order of possibility. But these possibilities do not ever have to become actualized to prove that they are real. "Being [essence] is a power, but is indifferent to the exercise of that power, since it is the power itself."

The universe of existence is the actual, the spatio-temporal. "Existence may be defined as whatever affects or is affected. Existence thus includes the definition of essence, but only in the active sense." It depends on essence for its existence.

"Destiny is the direction of existence toward essence." It is not a separate universe, but an interrelation between the other two.

These are presented in an elaborate and very detailed development of all the aspects: postulates, categories, types of structure, levels of being, degrees of integration. Since it attempts to be all-inclusive in scope, there are naturally included classifications of all the sciences and disciplines hierarchically arranged.

A systematic summary (pp. 663–684) is presented as a postulate set. These include for the universe of essence, 117 postulates; for existence, 160 postulates; for destiny, 123; and in addition, for the special case of epistemology, 111; for the "all-presumptive calculus," 88—a grand total of 599 postulates. In connection with this the author states: "The postulates of a system, while the chief elements, are not the whole system. In this treatise not all the elements of the *A–R* system are given, and those that are given are not presented systematically. . . . The claim of the *A–R* system is to be all of the truth available at this time, i.e. with the theories and facts now known. It will be supplemented and perhaps largely replaced as knowledge increases. Thus it claims to be the discovery of the truth but is fallible."

Nevertheless, without sacrificing the wide range of topics and material included, the book could profitably be greatly condensed. There is much repetition where the restatement in no manner advances the argument. In fact, as far as any substantial contribution to philosophical analysis is concerned, that could adequately and more effectively be presented in a brief essay.

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BRIEFLY REVIEWED

The World of Eli Whitney. Jeannette Mirsky and Allan Nevins. New York: Macmillan, 1952. xvi + 346 pp. Illus. $5.75.

THE title gives the reader promise of more than a biography. The tale centers, of course, about a Connecticut Yankee, but its setting interrelates cotton plantations of the South with the arms factories of Mill Rock, near New Haven, Connecticut. One expects the biography of a man but finds the text uncovering the evolution of an idea.

Born ten years before Bunker Hill, bereft of his mother at the age of five, the eldest of four children, Eli Whitney, Jr., early developed resourcefulness and self-direction. Schooling was incidental until at about the age of eighteen he expressed a desire to go to college. His preparation in academies and his self-support by intermittent teaching eventually earned his father's reluctant consent. During the final years at Yale that consent was confirmed by his father's financial help.

After graduation he accepted service as a tutor in a South Carolina plantation family. Actually, he never entered that service. Instead, he invented the cotton gin during a half-year sojourn in the home of General Nathanael Greene's widow near Savannah, Georgia.

For a period of years following, his time was given to alternate "tooling" for the gin's production, litigation over patent rights, and disheartening negotiations for working capital. This third need was the spur that presently prompted application to the newly formed federal government for a contract to manufacture arms for its army. In the struggle to meet the time

limits of that contract the principle of "parts production" for a sort of assembly-line pattern was evolved. In the practice of parts production he began the system of fabrication by "tools." In this he depended on his machines for the accuracy of the operation rather than on the skill of the workmen and so was able to employ unskilled labor for servicing the machines. In all of this one sees the elements of what later became mass production, so significant in the rise of such industries as those associated with the names of Colt, Singer, McCormick, and Ford.

Whitney's success in arms production for the government required contracts of long duration; political shysters often pulled him from his mills to lobby for his program of production. In these frequent sojourns at the seat of the national government he became well known to many of the nation's leaders, including the presidents, of that day.

Romance came late for the inventor. He did not marry until after the death of Widow Catherine Greene-Miller—perhaps because between General Greene's wife and Whitney there was "rooted a delicious friendship—that had flowered for twenty years." His own silence in regard to this aspect of his life leaves the reader only conjectures. At the age of fifty-two, he married Henriette Edwards, grand-daughter of Jonathan Edwards. She was twenty years younger than her husband. His eight remaining years were cheered by the birth of his three children and a happy domestic atmosphere.

The authors, the second of whom is well known and widely read, have very skillfully contrived to bring much of the account to the reader in the words of the letters from which the facts are drawn. In the words of the authors, their subject was a major contributor to the industrial progress of his country: "His genius, his skill, his persistence became puissant forces by which, in the developing Republic, unity defeated disunity, uniformity replaced diversity, technical expertness supplanted a haphazard rule of thumb, and Plenty was empowered to conquer Want."

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The Medieval Science of Weights (Scientia de Ponderibus). Treatises ascribed to Euclid, Archimedes, Thabit ibn Qurra, Jordanus de Nemore, and Blasius of Parma. Ernest A. Moody and Marshall Clagett, Eds. Madison: University of Wisconsin Press, 1952. 438 pp. Illus. $5.00. (Lithoprinted.)

THOSE who discuss the historical background of modern science and technology will often vault, mentally, from the achievements of Aristotle and Archimedes, of an ancient world, to the times of Galileo, Descartes, and Bernoulli. The accomplishments of the thousand years of medieval history are brushed aside as immaterial. Those, however, who know the medieval period best have discovered for themselves that the final four centuries of that period were not unimportant either to technology or to creative thought.

The book under review deals in a special and limited way with certain accomplishments of those four centuries, in western Europe, in relation to that part of the physical sciences known as statics. As a background for the presentation, there is swept in for the reader the effect of the rapidly flowering Arabic scholarship in perpetuating and carrying forward the older Greek ideas. Through the Moorish universities of Spain the knowledge of the newer Arabic mathematics, the Arabic number system, and a more dynamic approach to the problems of force and weight reached Christian scholastic teachers in the Latin west. The result was fruitful. In the early thirteenth century Jordanus de Nemore, of France, was a central figure in extending Arabic conceptions into the field of mechanics.

To establish clearly the accomplishments of the medieval scientific writers, the authors use the first 279 pages to present eight treatises on weights and related topics, ascribed to Euclid, Archimedes, Thabit ibn Qurra, Jordanus de Nemore, and Blasius of Parma. All are based upon Latin-written parchment manuscripts of medieval times. All have been carefully edited and are accompanied by adequate English translations. The first four of these treatises were available to, and the other four were produced by, scholastic scholars of medieval Europe. They may be considered, then, to give the background for, and the contributions made by these scholars to, the science of statics. To be found among the contributions are the general lever principle, that of the inclined plane, the composition of forces, and virtual displacements, with some suggestion of the use of the basal principle of work. All these have commonly been regarded as the accomplishments of later centuries.

The compilers, who are also the editors and translators, are quite modest when they write, "It is hoped that these texts and their translations will be of value to students of the history of science, and serve in some way to remedy the pitiful shortage of modern editions, and particularly of English translations, of medieval scientific writings in this field."

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ERRATUM

The first sentence in the third paragraph of Bentley Glass' review of *Understanding Heredity*, by Richard B. Goldschmidt, on p. 189 of the March, 1953, issue should read as follows:

"Those who know Professor Goldschmidt's eminence in the field of physiological genetics, and his "radical" views of the constitution of the genetic material and the nature of the mutational changes that participate in evolutionary processes, will be surprised to see that these radical ideas are scarcely alluded to. . . ."

Due apologies are made for an embarrassing printer's error.